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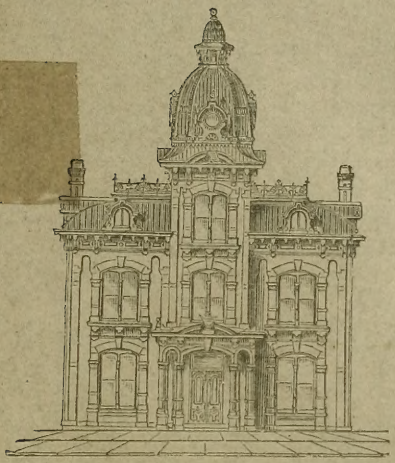

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

AND ITS

SCIENTIFIC SECTION.

POUGHKEEPSIE, N. Y.

1885-1887.

VOL. IV.

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TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

1885–1886.

NOVEMBER 10, 1885—TWENTY-EIGHTH REGULAR MEETING.

Forty members and two hundred guests present.

ANNUAL ADDRESS.

INTERPRETATIONS OF NATURE.

BY WILLIAM G. STEVENSON, M.D., PRESIDENT OF THE INSTITUTE.

Members of the Institute, Ladies and Gentlemen : As a sentient, perceptive, and self-conscious being, man has ever sought to interpret the phenomena of the world around, and to solve the mysteries of his own existence; and the interpretations which have been given at different times may be illustrated by the myths and legends of the past, by the scope and progress of speculative thought, and by the generalizations of inductive reasoning. These are the pictures in the gallery of thought, the visions—grotesque and sublime—which reflect man's ideal conceptions of nature.

To primitive man the world was only what the senses made it; and, in the absence of knowledge, the interpretations of nature depended upon the freaks of an extravagant fancy. The world was filled with mystic deities, made after the similitude of man, and beasts, and

birds; and the air, earth, and water teemed with unsubstantial gods—the workmanship of a capricious imagination.

To the Greek mind of Homeric days the earth was pictured as a small, flat, circular plain—extending from the inaccessible lands of the happy Hyperboreans on the north to the African deserts on the south, and from an indefinite India on the east to the Pillars of Hercules on the west; while the River Ocean, unruffled by storm or tempest, surrounded all. Fancy placed above this plain a brazen dome, in which were set the sun, and moon, and stars, moving in their courses under and around the disk-like earth.

To the north were the lofty mountains, from whose caverns the "piercing blasts of the north wind" rushed forth in their chilling fury, and where Cimmerian darkness covered all with the gloom of an eternal night. The enchanted islands of Circe and Calypso, and the floating island of Eolus, lay to the west of Sicily; and the entrance to the infernal regions was north of the Pillars of Hercules. The Elysian fields—radiant with summer and fanned by gentle zephyrs—were far beyond the known boundaries of the earth; where, too, were found the gardens of Hesperides, with their golden apples guarded by singing nymphs and a sleepless dragon. The sea—now known as the Mediterranean—divided the earth into two nearly equal parts: Greece was the central land, and Olympus, with brow above the clouds,—bathed in constant sunshine—was the mountain home of the gods.

With the growth of knowledge a restraining influence was thrown, by the reason, around the imagination; which, as if seeking to make amends for its own riotous workings by giving to the future imperishable records of the past, gathered its mystic deities from the field and sky, mountain and river; and, with the magic wand

of art, transformed them into speechless marble, or, with poetic fire, described them in immortal verse.

Mythology, with all its illusions, was, nevertheless, a legitimate result of the subjective method of interpreting natural phenomena. Man saw with the eye, and not through it; and sought to harmonize objective facts with ideal conceptions, which were based on neither accurate observation nor verified knowledge.

In this connection, it is well to keep in mind the important fact that correct reasoning and healthy imagination depend upon "accurate perception and exact memory."

"Knowledge is the perception of relations," and this implies ability to distinguish differences, which depends upon accurate observation.

An imperfect perception of an object gives to the mind false data, by which reason is distorted, and imagination—freed from the controlling influence which observation imposes upon it—degenerates into fancy. The earth becomes thereby a fairyland, peopled with chimera, satyrs, and sirens.

The subjective method reached its highest philosophical expression in the teachings of Plato, wherein we learn that objects are only inexact material embodiments of ideas,—"unsubstantial shadows" without reality, and, therefore, our knowledge of the external world is to be attained not by observation, but by simple reflection. The human mind, with all its varying shades of thought, thus became the measure of the universe, and the interpretations of nature rested, not upon the verities of the world around, but upon the conceptions formed when the mind contemplated itself alone.

This idealism, so flattering to the vain conceit of man, robbed nature of her unity and truth, and seriously retarded the progress of knowledge.

Gradually the tide of thought rolled on until Aristotle

—the loftiest mind of the ancient world—reversed the idealistic methods of the past and, on the basis of observed facts, proclaimed the laws of deductive reasoning. The known facts of nature were, however, too few to enable him to make a generalization which would, to any great extent, embrace the phenomena of the physical world, and, although he believed the earth to have a globular form, his speculative thought yet made it the fixed center of the universe, around which the sun, moon and other planets revolved.

To the master minds of the Alexandrian school, belongs the honor of first organizing and applying the principles of inductive science to the interpretation of physical phenomena.

Pure reason found its noblest expression in the science of mathematics, which the genius of Euclid had created; while the foundations of mechanics and hydraulics were firmly established by the discoveries of Archimedes. More extended knowledge enabled Erotosthenes to free geography from the mythical legends which encumbered it; and inspired him to attempt, even with imperfect data, to measure the circumference of the earth, while to the mental vision of Hipparchus the vault of heaven, hitherto so circumscribed, expanded into the amplitudes of space; wherein, as a mere atom, the earth was seen rotating on its own axis as it moved onward around the sun.

Notwithstanding the achievements of the intellect, and the perceptions—faint though they were—of law and order in the material world, Greek philosophy had no revelation concerning man himself, which satisfied the human heart, and, although for a time sustained by the brilliancy of Arabian culture, it did not survive in the presence of the new faith, which, with exalted idealism, taught the brotherhood of man, and enticed all by the promise of a happy life hereafter.

The ethics of Paganism were outranked by the ethics of Christianity, and religion contested for supremacy over philosophy.

Unhappily, in its efforts to ennoble man, faith absolved itself from reason and alienated the intellect by denying liberty of thought to the human mind. The rich legacy of truth, which had been received from the Pagan world, was cast aside, and nature was no longer interpreted according to any standard of verified knowledge, but by scripture texts which were made to harmonize with the abstractions of the monastery. Belief stifled inquiry and dogma supplanted reason, and, for over a thousand years, an ignorant credulity dominated the human mind and intellectual darkness brooded over the world.

This was, however, but the ebbing tide of thought, not its death; the transformation of reason into emotion; the materialization of faith into the objects of its adoration. The principle of conservation applies to mental as well as to physical affairs, and "fosters the eternal vital process of advancing reason."

The germs of knowledge, which had been scattered from the intellectual watch-towers of Athens and Alexandria, of Grenada and Cordova, came floating down through the murky atmosphere of the middle-ages, until, under the energizing influences of the great geographical discoveries of the fifteenth and sixteenth centuries, they developed into a grand renaissance of thought, and it was again demonstrated that the world was more than the senses made it, and that the horizon of truth extended far beyond the mental perspective of the past.

When the anchor dropped from the prow of Magellan's ship in the harbor of San Lucar, it carried with it to the bottom of the sea the false Patristic doctrine that the earth is flat. Then it was that the picture, which nature

herself had so many times painted upon the moon during an eclipse, was understood ; the shadow was a messenger of light ; a revelation of a truth ; an appeal to reason.

Del-Cano had completed the circumnavigation of the earth, and verified the fact that it was indeed spherical in form. Then the objective world, so greatly enlarged by the navigators of the age, "began to assume a preponderating force over the mere creations of the mind." New lands and seas with forms of life so strange and wonderful, new skies and stars filled the mind with an enthusiasm which found expression in the marvelous advance of mathematical and astronomical science in the seventeenth century, by which Copernicus was enabled to remodel the Ptolemaic system, and Kepler to formulate the laws of planetary motions, while Newton, interpreting these laws, bound with the force of gravity the swinging atom and the rushing world. Alhazen had known this force as it applied to terrestrial matter, but it was the glory of Newton to see it holding the universe in the bonds of unity, and to determine the measure and method of its action.

The telescope of Galileo brought to human vision worlds hitherto unknown, and the "discovery of Jupiter's moons marks an important epoch in the history of celestial physics ;" for the shadow cast by the occultation of these moons became the revealing messenger of light's velocity, and upon this knowledge depends "the interpretation of the aberration ellipse of the fixed stars in which the great orbit of the earth in its annual course round the sun, is, as it were, reflected on the vault of heaven."

A century later, more powerful lenses broke the dim, blending light of nebulæ into multitudes of glittering suns, while the spectroscope has gathered the waves of

light from the secret chambers of distant space, and from them learned the history of the stars.

The imagination of Laplace, leaping beyond the limits of sensory perception, saw cosmic matter "without form and void" integrating into worlds, and law and order were seen reigning throughout the material universe.

Astrology had been supplanted by astronomy. While physical philosophy was thus engaged in measuring space and magnitudes, and interpreting the laws of cosmic phenomena, its companion science, chemistry, was slowly putting aside the swaddling clothes of alchemy, which had so long—though vainly—sought to transmute the baser metals into gold, to discover the philosopher's stone, and find the elixir of life. Alchemy did not analyze substances; it simply decomposed them: and it had no knowledge of the ultimate composition of the different forms of matter.

Paracelsus and Van Helmont, even Boyle and Stahl, although innovators of the "sacred art," were but the "incubators of chemistry." And not until the balance and the test-tube, in the hands of Black, Priestly, Cavendish, and Scheele, had, by analysis, laid the foundation of quantitative chemistry, was the hope justified that the intrinsic properties of matter, and the laws of atomic combinations, would be revealed.

New facts, through analysis, accumulated, until Dalton, in 1804, suggested the atomic theory as a new means of interpreting phenomena. This theory was soon promulgated under the generalization known as the law of Avogadro or Ampère; and from this dates the birth of modern chemistry.

When Rumford and Davy experimentally demonstrated that heat was but a "mode of motion," and when Joule, in 1850, determined its mechanical equivalent, and established the law of thermo-dynamics, it was for the first time possible to make a classification which

would embrace the known facts relating to heat, light, electricity, and magnetism. Then also came the deduction of those universal laws known as the correlation and conservation of energy.

The outlines of the earth's surface had been sketched by the navigators of the fifteenth and sixteenth centuries, but the history of its structure remained unknown. There was no one who could read the record of the rocks, or describe the method by which its valleys had been dug, its mountains raised, and its architecture made so grand and beautiful. The pictured forms of life upon the stone—Whence came they? What are they? By what cunning device did nature's brush paint them, or her burin engrave them, or her chisel carve these forms so real and life-like? How came these casts of organic things, arranged with special types in special places, and what is the chronological order of events?

It was not until the early part of the present century that any systematic effort was made to interpret any of these things. Theories there were, but they had no basis in facts, and reflected only the troubled dreams of the astrologer who, displaced from the heavens by the telescope, found for a time a hiding place in the recesses of the rocks, and in the caverns of the earth. Within the short period of half a century the increase of geological knowledge has revealed the mysteries of the earth and told the story of its creation.

The revelation of truth thus "gathered from one small sphere, is the deciphered law of all spheres," and evidence is given that the principle upon which rests the interpretation of all physical phenomena, is matter affected by the uniform and constant operation of cosmic forces.

Notwithstanding the progress made in accumulating and generalizing the facts relating to animal life, our

knowledge is as yet too imperfect and fragmentary to admit of a classification of biological phenomena which will represent a complete history of adult life as related to its personal and ancestral development. So long as the dogma of the "constancy of species" was accepted —which "excludes any natural relationship" between different species—it was possible to make a morphological classification which would satisfy the intellectual demands of the days of either Aristotle or Linnæus.

The sciences of comparative anatomy and embryology were then unknown, and such important factors, in the analysis or synthesis of organic life, as "differentiated," "homologous" and "rudimentary" organs had no meaning; neither was there knowledge of the transforming influence of environment nor of the potency of inheritance, in fixing and transmitting the changes wrought in the organism.

Cuvier knew not the methods of development, but only the anatomy of the matured structure. His investigations, however, proved the existence of extinct forms of life, whose characters were intermediate between distinct groups of existing forms, and therein were laid the foundations of palæontology.

Bichat inaugurated the study of tissues, and the functions or dynamics of the bodily organs.

Von Baer, in 1827, discovered the mammalian ovule, made embryology the basis of anatomical classification and established the biological "law of differentiation from a general towards a special form;" while Agassiz showed that "in some cases, the older forms preserve, as permanent features, structural characters which are embryonic and transitory in their living congeners." Then came the patient toil of Darwin in gathering, analyzing and classifying the facts which nature so lavishly furnished to him, until he was able to enunciate the theory of variations in nature through "natural se-

lection," and formulate the laws of biological evolution. This generalization, resting as it does upon the truths of inductive science, gives to the mind a picture of nature's unity, completeness and design throughout the cycle of life.

Thus has observation caused the belief that the phenomena of the world are not isolated, but related; and that its methods are not capricious, but orderly.

The idea of "law" has shadowed that of special interference, and a broader classification deals with relations rather than things.

The relations existing between the worlds of matter and of life, which more completely show the unity of nature, are not necessarily determined by the physical or functional character of the objects related, nor by the origin or mode of development of their physical structures; but by the finer and deeper relations of adaptation and adjustment by which the action of physical forces upon the organism are necessary for the expression and maintenance of vital phenomena.

The pull of gravity, which defines the orbits of the stars and bounds the ocean's tides, also influences the beating heart and the flowing blood. So delicately has nature adapted organs and adjusted forces, that the pulses of the air break upon the ear as sound, and ethereal tremors are interpreted by the mind as heat, light, and actinic force.

From the sensations resulting from the impressions made by external physical forces spring ideas, feeling, will; and in the classified relations of such profound differences is found the evidence which establishes the highest form of nature's unity.

Here ends the power of inductive science. It explains the phenomena of matter and of life, by classifying facts and by giving a formula for an "entire order of phenomena;" but it does not explain the verities that

lie behind the phenomena which are presented to the senses or perceived by the intellect.

The course of the falling stone or the blazing star is explained by "gravity"—but what is gravity? and chemical affinity and life—what are they? We recognize some of their passing effects, but their essential natures are riddles we cannot solve.

When nature is interrogated she answers—not in words, but signs; the rocks give record of her years, and the skies measure the infinitudes of her vastness; she emerges from the deep darkness of the past and, in her giant strength, throws into space revolving worlds, and marks their courses by the rays of light; she paints her beauty on the hills and vales, pours sweetness in the flowers and fills the earth with joy and gladness; but, though closely scanned, nature reveals not the secret of her power—the where, the whence, the whither of her life.

The mystic threads of energy and matter, which have been so skillfully woven into the grand fabric of the universe, have been traced by the human intellect through many forms of life and worlds; but the power that spins these threads and puts them upon the shuttles of nature is not discerned by the microscope nor telescope, neither can the balance measure the wisdom that has designed the symmetric beauty of the world. That, however, which lies beyond the jurisdiction of inductive science, speculative thought seeks to reach through the still broader generalizations of philosophy, which represent the highest synthetic achievement of reason.

It was the abuse of the deductive method, the attempt to reason, either without objective facts or from data that were not true, which brought it into disrepute; and the abuse of the inductive method—the toleration of an unwarranted tyranny of the senses over reason—can only retard mental progress.

If, in its self-asserting pride, the human mind has, at times, closed the avenues of the senses, through which the physical world becomes known, and, as a result of the abuse of the deductive method, declared that the earth rests upon the back of a turtle; that, in the beginning, all life forms sprang from nothing into perfect being; that the ocean's tides are but the breathings of the live monster-earth; that comets are heralds of divine wrath to an impenitent world; that eclipses are expressions of nature's grief because of some human calamity; and that no steam vessel could ever cross the Atlantic—if such gross errors are to be ascribed to the deductive method, it is, likewise, true that the path of inductive reasoning is strewn with the wrecks of discarded theories, imperfect observations, and erroneous interpretations of facts in nature.

When inductive science, exulting in its brilliant achievements, points to the starry heavens and tells the revelations it has made, it remains for the deductive reasoning of Newton to declare the universal law of gravitation, by which atoms aggregate to suns and worlds are held in their orbits. If the telescope of Herschel discovered the planet Uranus, it was, nevertheless, the calculating, deductive reasoning of Leverrier that located Neptune far beyond. Inductive science may, by means of the spectroscope, read in the rays of light the history of the stars, but it was the glory of Laplace to proclaim —through deductive reasoning—the laws of cosmic evolution, and to trace the changing forms of matter from the glowing vapor to a solid world. Inductive science may justly claim high honor for many and marvelous discoveries in the domain of organic nature, but the grand generalization of life phenomena, under the formulated laws of biological development, is a masterly deduction from inductive facts.

Scientific methods are largely inductive, and apply to

the "various departments of phenomena" in all their parts and reciprocal relations as antecedent and sequence; while philosophy is deductive in its method, and seeks to group the orders of phenomena into a cosmic whole, and learn the laws of their relations.

There should be, therefore, no reprisals between these two methods of attaining truth, for they are mutually dependent, and, in their co-operative action, reflect the highest possibilities of thought.

Notwithstanding the triumphs of mind in the realms of material phenomena—it has not transcended the boundaries of relative knowledge, nor lifted the veil that hides the omnipotent power of the universe. Toward the unfathomed mysteries beyond, the philosophic attitude of the mind is one not of deep assurance but of calm expectancy, and the question is yet asked, as by the Roman procurator of old—"What is truth?"

Philosophy has no key which can reveal the truth of things in themselves, "but only as they are related to our intelligence." Neither can the finite understanding know the infinite; yet as the grand body of truth embraced in geometrical science starts from axioms which are assumed to be true, but which can not be so demonstrated,—so philosophy—while it cannot show the essential nature of the substratum which constitutes the basis of objective realities,—cannot even "prove the existence of an external world,"—it nevertheless postulates the existence of an Ultimate Reality that transcends the conceptions of the mind, as the one essential factor for all philosophical reasoning; and, by logical processes,—the mind sweeps its curves of thought along lines of least resistance, until every where in nature is felt the influence of an intelligent power, whose existence cannot be denied, although its essence may not be defined.

This intelligent power is God.

If, in a moment of careless thought, any one should feel that a deduction which proclaims the existence of such a power is an absurdity—because it, at the same time, admits the impossibility of defining its real nature —let him reflect upon the facts in the physical world which are demonstrated as true, but are, nevertheless, incomprehensible, also.

Light, for example, in its physical aspect, is a vibratory movement of a material medium called "æther," which transmits the tremors caused by a luminous body at the rate of one hundred ninety thousand miles in a second. In the scientific use of the imagination we see this ether filling every space between worlds and molecules—pervading everything "as freely as the air moves through a grove of trees." We see the particles, which, as attenuated forms of matter, constitute this ether, in motion. These ethereal tremors, these dancing atoms, impinge upon our senses, and are revealed to us as radiant heat, or light, or actinic force, according to the rapidity of their motion.

The colors of the visible spectrum—which give such variegated beauty to nature—also depend upon the number of times, per second, these ethereal waves beat against the eye.

What, then, is this medium called ether, which thus throbs and pulsates, and transmits the thrill of worlds with such velocity? How shall we define that of which the senses give no knowledge ; which has neither smell nor taste, and can not be felt, heard nor seen? Shall we speak of it as matter when, to the senses, the known properties of matter cannot be found? Let us call it the vapor of cosmic atoms which defies the senses, and remains an unmeasured factor in the equation of the universe.

By processes of pure reasoning, that which thus

eludes the senses, is, nevertheless, proved to be a material reality—although its nature has not been defined.

The phenomena of light, as interpreted by the undulatory theory of Young, establish the fact that "light is not a substance, but a process going on in a substance;" and, reasoning from verified data, proof is given that this substance which is capable of transmitting energy, must itself possess the physical properties of elasticity and density, and, therefore, be a form of matter.

But what form? Here analogical reasoning leads to uncertainty; for a medium that fills all space and transmits energy, even in an atmospheric vacuum, suggests the idea of "extreme tenuity," which indicates a "density that is almost infinitesimal;" this means that the particles, which constitute this medium, are, themselves, not only "mathematical zeros in weight," but, also, that they are separated from each other by regions of empty space. A medium having such a molecular constitution, however, cannot transmit the energy of light, and this contradiction of facts necessitates a rejection of the idea of a medium of extreme tenuity. On the other hand, the fact that light is transmitted at the rate of one hundred ninety thousand miles in a second, compels us to regard this transmitting medium which—to the senses—is so "impalpable, invisible, and imponderable," as in reality "a medium infinitely more compact than the most solid substances which can be felt and weighed." It forced Sir John Herschel to conceive it not as an air, nor as a fluid, but as a solid—"in this sense, at least, that its particles cannot be supposed as capable of interchanging places, or of bodily transfer to any measurable distance from their own special and assigned locality in the universe;" this would give to this medium a constitution not molecular but continuous, and make it a solid whose density is greater than steel.

The earth is thus united to the worlds in space by

bridges of adamant, which bear to and fro the messengers of light and radiant heat with the rapidity of thought.

We know, therefore, that a cosmic medium—called ether—exists because of certain definite effects produced by its agency; but, beyond this, the real mystery remains.

If, therefore, reason is able to establish as a material fact something which cannot be observed by the senses, and which is known only because certain physical phenomena cannot be explained without its agency, there is nothing "absurd" in deducing from the facts in nature the existence of an intelligent, creative power in the universe "in whom we live, and move, and have our being." This deduction is a lawful product of reason, a legitimate postulate of philosophy, when the evidence of an intelligent power, as manifested in "design in nature," is fully analyzed.

Such has been the general belief of man, although entertained as an article of faith rather than as a conviction established by evidence.

When, therefore, modern science, by its discoveries, readjusted the relation of things, and revised the interpretation of phenomena, it forced faith from its ancient moorings, and disturbed thereby the continuity of thought and of belief.

In the confusion and uncertainty naturally attending this readjustment of theories and beliefs, some there were who bounded the possibilities of truth by the narrow limit of their conception of things; and assumed—for they could not prove—that matter and physical force—unconditioned—were the only agencies required in the evolution of worlds and life.

This is a philosophical absurdity, because, in alleging self-existence, causation and beginning are denied, which is unthinkable.

Others with broader thought—although having no knowledge of life independent of matter—exclaim with Tyndall—"Here the vision of the mind authoritatively supplements the vision of the eye. By an intellectual necessity I cross the boundary of the experimental evidence, and discern, in that matter which we, in our ignorance of its latent powers, and notwithstanding our professed reverence for its creator, have hitherto covered with opprobrium—the promise and potency of all terrestrial life." But listen—he further says—"Our states of consciousness are symbols of an outside entity which produces them and determines the order of their succession, but the real nature of which we can never know. In fact the whole process of evolution is the manifestation of a power absolutely inscrutable to the intellect of man," "as little in our day, as in the days of Job, can man by searching find this power out."

This is an expression of intellectual integrity exceedingly refreshing to reflecting minds, for, while within the limits of the material world everything is seen originating from physical antecedents, by methods so uniform and invariable that we call these methods "laws," there is nevertheless an existence of some reality, some inscrutable power back of all matter and all force, in relation to which no predicate can be formed.

Philosophy and science are in accord concerning the existence of a reality beyond the material expressions of nature, as revealed to the human mind, and it remains to ask if this reality is an intelligence that operates, as an efficient cause, through definite means to the attainment of definite ends?

This must be answered by the evidences of design as manifested in the universe.

The testimony given by nature is, however, susceptible of different interpretations according to the standpoint of the observer, which enables him to behold

either the whole or only a part of the series of transforming views as the grand pageant of life moves by. Hence it is that our ideas of final causes have been modified as knowledge has increased, and hence, also, our convictions concerning design in nature have been shaken by the facts upon which the theory of natural selection rests.

Laplace thought that there was no room for final causes in the presence of data which enable us to explain problems scientifically, and Dr. Whewell, the champion of design, thinks it is no longer applicable to the inorganic world.

I confess I do not understand the logic which denies design to physical phenomena, simply because the inherent properties of matter, and the laws of energy construct a universe without need of special interference.

A broader view will see the design far back of that which is incident to our mental vision, at creation's dawn when steadfastness was first given to the atoms from whose combinations the universe was built, and which, amidst the crash and ruin of worlds, shall "remain unbroken and unworn."

Nevertheless, since its own champions have withdrawn design from the domain of inorganic nature and placed it in the keeping of organic forms, let us examine some of the evidence used for and against its retention in the evolution of life. For it, and constituting its strength, is the evidence of adaptation and utility of special organs, for special purposes.

So profound has been the conviction that an intelligent, designing mind is necessary "for the contriving and determining of the forms which organized bodies bear," and that each organ has been specially and directly created for a definite function, that Paley unreservedly rested the entire question of design upon the facts of human anatomy.

The eye, the ear, the hand—each organ was made in all its present perfection for a special and individual purpose; and, as the mechanism of a watch proves the existence of design—and a designer, so the more wondrous mechanism of the bodily organs proves that they are the immediate products of a creating, designing mind.

Admitting, as I do, that forethought is apparent in the structural plans of life, and that there is evidence of contrivance in the harmonious adjustment of all its varied phenomena, it is, nevertheless, impossible to understand design—reflecting as it does the last expression of antecedent purpose—until the interlacing threads of law have been traced hither and thither through their many and complex relations.

The discovery of the relationship of facts which bear witness to the unity of nature should, therefore, precede all deductions pertaining to design. What idea, for example, of adaptation or design could have been entertained by Aristotle when he ignorantly stated that lions and wolves have but one neck-bone, for the reason, he says, that "nature saw that these animals wanted the neck more for strength than for other purposes." To try to tell why nature has done a thing is, at any time, an intellectual performance of doubtful merit, but to try to do it in the absence of definite knowledge of related facts is an extravagant violation of intellectual integrity. Design in the structure of the solar system was hardly a debatable question previous to the times of Kepler and Newton.

The weakness of the argument of design has always been that its champions have failed to trace the methods by which nature has wrought the mystic patterns of living things, or to recognize the power of the inherent laws or properties of matter, and the influences of envi-

ronment which are necessary and modifying agencies in the teleology of life.

In the interpretation of these silent powers Darwin became the seer of the age. The facts gathered by his patient toil have revolutionized thought, and substituted, in the place of special creation and immediate design, the conception of necessity through the operation of natural laws, whereby ends are attained of which no prophetic utterance can be made. This excludes "the agency of an intelligence in which the image or idea of the end precedes the use of means," and is, therefore, in seeming conflict with the view commonly entertained.

The doctrine of design, as commonly understood, fails, however, to explain too many things to justify us in longer accepting the narrow limits of its application, or to believe that human anatomy alone proves its truth.

It fails to explain the progressive stages of human anatomy during its embryological period, and its deviations from the common bodily construction, or to tell why it is that deformities, imperfections, monstrosities or reversions occur.

Where is the purpose, or what is the design in those human monsters, those brute-appearing forms, which come from some arrested development during embryonic life? Or in those anomalous conditions—existing even in adult life—wherein bodily organs betray their lowly origin? Where is the purpose and what is the design in those abortive or rudimentary, or homologous organs found throughout the vegetable and animal kingdom, whose apparent uselessness makes them strong witnesses against "design" in their existence? If "adaptation and utility are the marks of design," what shall be said in relation to those organs of human anatomy which are entirely useless in man, although of use in some lower animals? What is the purpose or design in the extravagant waste of seed, eggs and germs—from

which plants and animals come? Why should a million perish and but one survive?

I do not stop to name the individual facts, illustrative of these points, which speak so strongly against design and present difficulties which are insuperable on any theory of the direct and independent creation of species.

Notwithstanding this negative evidence against design, I am convinced that it is a truth and that an intelligent power works through definite means to the attainment of definite ends; and the reason why we so often err in our conceptions of this truth is because we can not always determine from an act, or a result, the purpose or place associated therewith. We see a boomerang thrown forward, but this act does not reveal the purpose of him who threw it, which was to strike an object standing in an opposite direction; this purpose can only be inferred when we know the deflecting force which is inherent, when a stick having peculiar curves is thrown in a certain manner.

The position of the rudder will not alone enable us to tell the exact direction of the boat, for the pilot has estimated the deflecting force of winds and tides and placed the helm in accordance therewith. These deflecting forces are known as natural laws, whose method of acting it is the object of science to determine; they are the influences of environment and inheritance—those silent but ever-present potencies—which modify organic forms and conceal the direct evidences of design by the complexity of their operations. When, therefore, I speak of "necessity,"—in contradistinction to "design,"—as the directing force of means to an end, I mean simply the action of these secondary causes: those which exist in the nature of things; the natural laws of the universe; the inherent and inexplicable properties of elementary and organized matter; the phenomena which may be compassed by the human intellect.

In seeking to explain these laws—the growth of living things, the changes wrought by natural selection and reversion, the conflicts of life in the pressing struggle for existence, the survival of some and the destruction of many—the doctrine of evolution seeks only to interpret phenomena and the laws of their succession; to know methods, not purposes. It enters not into the secret councils of creative cause. Its teachings seem to me eminently teleological, although, in the action of secondary causes, they regard mechanical necessity alone. By the operation of these natural causes, the explanation is found for the many apparent contradictions of design.

It is, indeed, true that evolution alters our conceptions of the relations of a creative and superintending intelligence in the world; but it does not deny its existence. Indeed, as I have already stated, such an existence is the first and most essential postulate of all philosophical reasoning.

Professor Huxley well says: "Perhaps the most remarkable service to the philosophy of biology rendered by Mr. Darwin is the reconciliation of teleology and morphology, and the explanation of the facts of both, which his views offer." "The teleological and mechanical views of nature are not, necessarily, mutually exclusive. On the contrary, the more purely a mechanist the speculator is, the more firmly does he assume a primordial molecular arrangement, of which all the phenomena of the universe are the consequences; and the more completely is he there by at the mercy of the teleologist, who can always defy him to disprove that the primordial molecular arrangement was not intended to evolve the phenomena of the universe."

Adaptation, contrivance and design—even as we know them—relate to the operations of mind, and the logical continuity of thought requires us to ascribe them—when

seen in nature—to some thinkable antecedent, even if this lies within the realms of that "unseen universe" which, although unobserved by the senses, is so real to the conscious soul.

This thinkable antecedent can only be an intelligent power, and to believe that from its action, the physical and biological order of nature proceeds, is, as it seems to me, an intellectual necessity.

This belief brings no "permanent intellectual confusion," for, in confirming the law of biogenisis that life springs from life, it gives to nature a higher unity and energizes it with a vital principle, a universal intelligence which is the efficient cause of all things.

And so we come to recognize the transcendence and immanence of a supreme mind in nature, upon which, as an eternal foundation, rests the visible universe.

This is the unfathomable mystery of being, concerning whose nature science can formulate no theory and philosophy can give no definition, but of whose truth and reality there can be no reasonable doubt.

For the evidence which thus convinces the intellect we are indebted to modern science and philosophy.

This fact alone should be a sufficient refutation of the charge so often and so ignorantly made, that the tendency of modern science is "materialistic"—implying that mind, in all its manifestations, is simply a product of brain matter, which lives and decays with the physical organization which gives it birth, and, therefore, has no existence after bodily death.

Such a statement contains a measure of exactitude, but it represents only one side of a dual truth.

The statement embraces two distinct propositions—one pertains to a verifiable fact and, therefore, comes within the purview of science ; the other relates to a deduction, which cannot be proved in an affirmative manner and falls within the scope of metaphysical philoso-

phy. It is a verifiable fact that every expression of consciousness,—every sensation, thought and emotion—is manifested through, and inseparably associated with a nervous mechanism; it is also a fact that every modification of this mechanism, every change in its material conformation—whether wrought by the favorable processes of development or by the repressive influences of injury or disease—affects the expressions of mental life, and thus favors the belief that the conscious mind "has its correlative in the physics of the brain."

Thus far, science feels its way secure, but it ventures no further; it beholds the "association of two classes of phenomena"—but it gives no explanation of their "bond of union." "We do not possess," says Tyndall, "the intellectual organ, nor apparently any rudiment of the organ, which would enable us to pass, by a process of reasoning, from the one to the other. They appear together, but we do not know why. Were our minds and senses so expanded, strengthened, and illuminated as to enable us to see and feel the very molecules of the brain; were we capable of following all their motions, all their groupings, all their electric discharges, if such there be; and were we intimately acquainted with the corresponding states of thought and feeling, we should be as far as ever from the solution of the problem,—'How are these physical processes connected with the facts of consciousness?' The chasm between the two classes of phenomena would still remain intellectually impassable." "The passage from the physics of the brain to the corresponding facts of consciousness is unthinkable."

That, which thus defies the analytic methods of science, is presented to the mind of the metaphysical philosopher as an independent and immortal soul, having the attributes of personality. This shadowy but real being is associated with and made manifest through the

human brain; while feeling, action, thought, directed by an imperial will, come upon the stage of life as the products of its working.

There is, however, no fact known to inductive or deductive reasoning which authorizes the conclusion that, because the brain is the organ of thought and feeling, therefore, they shall perish when it is dead.

We cannot, it is true, "have direct evidence as to the soul's survival until we ourselves die;" "but a negative presumption,' says Fiske, 'is not created by the absence of proof in cases where, in the nature of things, proof is inaccessible."

It may be that

> "God never meant that man should scale the heavens
> By strides of human wisdom."

If man be regarded simply as a "local incident, in an endless and aimless series of cosmic changes," it may well be thought that "that which befalleth the sons of men befalleth beasts" . . and "as the one dieth, so dieth the other." But we have seen that cosmic and biologic changes are not aimless; and that an omnipotent intelligence directs the affairs of the universe. Therefore are we persuaded to believe that, while man—so "fearfully and wonderfully made"—is, in his bodily perfection, the crowning glory of organic evolution, the human mind—in its actualities and possibilities—is the consummate flower of creative purpose.

The mind as a "part of the system of nature" is "specially adapted to the purpose of catching and translating into thought, the light of truth as embodied in surrounding nature," and consciousness is a "momentum of thought" which, as Spencer says, "carries us beyond conditioned existence to unconditioned existence."

Independent of any philosophic negations, the logic of moral probabilities is that immortality awaits the human soul.

> "It must be so. Plato! thou reasonest well,
> Else whence this pleasing hope, this fond desire,
> This longing after immortality?
> Or whence this secret dread, and inward horror,
> Of falling into nought? Why shrinks the soul
> Back on herself and startles at destruction?
> 'T is the divinity that stirs within us;
> 'T is heaven itself that points out an hereafter
> And intimates eternity to man."

The address elicited an interesting discussion by the Revs. Van Gieson, Elmendorf and Loomis; Drs. Warring and De Garmo, and was closed, as follows,

BY REV. H. L. ZIEGENFUSS:

Through the courtesy of our president I was allowed, two days ago, to become acquainted with the contents of the paper that has just now been read before you. If we differed more it might enhance the interest in this evening's discussion, but that we agree so nearly is, to me at least, a cause for sincere congratulation.

It is very gratifying to note on all sides that, as studious men delve down deeper into the depths of nature and become the better acquainted with nature's laws, their conclusions are couched in terms taken from the psychological domain quite as much as in those taken from the vocabulary of the pure physicist.

Some years ago Dr. Büchner wanted only matter and force for the creation of the universe, and of all that therein is. With that as a text he preached the gospel of materialism. It was an attractive gospel. It made things so clear that the uneducated could easily understand everything. Man had but a span of life, which began and ended here on earth. Death ends all. The

only immortality there can be is that of permanence of effect. Not in his spirit but in his deeds man is immortal.

Whatsoever of truth materialism has to offer we cheerfully accept. We endeavor to be hospitable to the true sons of science. We will acknowledge fact wherever it may be found, and follow it whithersoever it may lead. But the same course of conduct we demand of others. We beg them not to be narrow and exclusive in investigations, contracted in their sympathies, nor blind to certain other obvious facts of nature, even if they be not written down in the book of the chemist or the physicist.

For the sake of argument we, also, will begin with matter and force. But what do we really know about these? Matter, we are told, is that which affects our senses. We are conscious of the effects, but what do we know of the cause? We know certain of the properties of what we call matter, but nothing whatever of its ultimate nature and internal atomic constitution. We take a block of ice. It is a solid; it has the characteristics that we call weight, density, rigidity, color, coldness, translucency, and so on; but these tell us nothing whatsoever of the thing in itself. Apply heat and the solidity disappears; it becomes a liquid; rigidity is gone, and transparency takes the place of translucency; warmth succeeds coldness. Apply more heat, until the condition called steam is reached. Now our aforetime solid matter has become entirely invisible, having apparently lost all those qualities that it had in the form of ice.

Or, by electrolysis, let us split up the molecules of water into its components. On the one side we get oxygen, on the other, hydrogen. But this hydrogen, for instance —what is it? It also, itself, though at ordinary temperature it is a wondrously attenuated gas, has been forced into the liquid, and even into the solid shape. What is

it? Is it really a simple element? Or will science yet declare that this primal monad of chemistry is, after all, a compound? There are such indications. And is it really of the nature of a metal—as Graham would have us believe? The indications all lead towards that conclusion. It must be confessed that absolutely we know nothing whatever of its real nature.

And so it is in reference to force. We talk glibly of adhesion, cohesion, gravity, heat, light, magnetism, electricity, chemical affinity, and the like; but we know no more about them than we do of the correlation of thought and brain-matter. We look up at the moon and tell the child that the strong arm of a force that we call gravity holds that satellite a-spinning around the earth. But who of us has any adequate conception of the greatness of that force?[1] Round Top, the highest peak of the Catskill mountains, rises three thousand eight hundred four feet above the level of the sea. Lay along the side of the range a bar of steel one mile square. The upper surface of that steel bar will be one thousand four hundred seventy-six feet above the highest point of Round Top. Does that mile-square bar of steel represent the force which is needed to hold earth and moon together? No; it takes, not one such bar, nor one hundred such bars of steel, but eighty-seven thousand?

Let us say then that "in the beginning" there were only these two mysteries, matter and force. Let us go back into the early history of this globe. How strangely well for our comfort and highest well-being these strata of the earth's crust were laid,—happened to be laid. Then, as the conditions became favorable, there came life,—let us say the life represented by a solitary cell of protoplasmic or ante-protoplasmic matter. But what strange potency was there! Matter assumed new

1 See Savage's "Belief in God," p. 34.

form and manifested new force. True, in its first stages, life was of the lowest grade, but it was a new phenomenon. There came the divergence between animal and plant life. The seaweed came, and the moss, the fern, the conifer, the cycas, the palm, and the angiosperm; and parallel with these came all the various phases of animal life from the lowest unto the highest. Movement was progress. The appetency was strangely forward and upward. Then came thirst and hunger. Waste called for repair. There was struggle for existence. There was war in those days. Appetite and instinct, born of experience, were made allies. All points of advantage, whether of position, or movement, or color, or form, were quickly siezed. Force gave way to cunning, and cunning gave way to intelligence. From faintest glimmerings came the great warmth and carefulness of love. There came with the soul of man strange fears and hopes. The feeling of devotion struggled to utter itself through him. His soul became restless, and it was ever lured forward by unutterable ideals. And these phenomena of appetite and passion, of love and hope, of self-sacrifice and devotion are facts of nature just as really as are centripetal and centrifugal forces, as are heat and cold, as lightning-flash and auroral flush. Now why has this on-moving change in matter and force ever been upward? Why is there a "survival of the fittest?" Why is it that that which we call good and right is always synonymous with well-being? Everywhere and at all times the irrepressible conflict is doomed to work out emancipation and amelioration. The manifest destiny points to perfection and blessedness. Why?

Think of it again. In that primal, tenuous fire-mist, were not only the sun, and its planets, not merely this globe with its granitic mountains, the weight of its mobile oceans,—not merely the keenness of frost, the

flickering of auroral lights, the iridescence of snow-flake and ice-crystal, of water-mist and the nacreous sheen of sea-shells, but also matter most strange in constitution and marvelous in dynamic attribute, the colloidal and vital, the self-determining force of the living thing, the beauty and the cunning of bird and beast, the sweet incense of violets and honeysuckles, the potency of thought and love, the heart-stirring words of the prophets, the strivings of man towards freedom, and justice, and moral greatness, the deeds of Runnymede and Yorktown ; yes, there in that fire-mist were the germs of the fairest fruitage of man's spirit—the thunderings of express-trains ; the click of the telegraph, the ceaseless rumbling of the press, the sweep of the telescope, the keen searching of the microscope, the revelations of the spectroscope, the ideals of Raphael and Milton, the myriadmindedness of Shakespeare, the downrightness of Browning, and the "sweet names and foolish nothings" that the young mother in happy home croons to her smiling babe—all these were in that primal fire-mist !

This outcome of earth-history and man-history was "either foreordained in the germ,—in which case, essential Theism with its logical accompaniments is granted,—or produced by a spiritual environment, involving at least as much as we mean by Theism." [1] If it be granted that the light of the sun gradually called forth the eye, then we dare not hesitate to maintain that in like manner a spiritual stimulant called forth the forward look of hope and the upward look of faith and of devotion. Pagodas, mosques, cathedrals, hospitals and asylums, are as real facts of nature as are coral islands and coal-measures. They are each the resultant of force, ultimately of one force, which is progressive and ever ameliorative. It is a force that eternally makes for perfection, for well-being, for truth, for greater intelli-

1 Savage : "Belief in God," p. 168.

gence, for most loving kindliness of disposition and of conduct,—a wise and beneficient force—"a power not ourselves which makes for righteousness," in which we live and move, and have our being—God.

DECEMBER 1, 1885—TWENTY-NINTH REGULAR MEETING.

William G. Stevenson, M.D., president, in the chair; twelve members and three hundred guests present.

James M. DeGarmo, Ph. D., gave an address, entitled "The Quakers and their Doctrines," which was discussed by Messrs. Heermance, Swan and Stevenson.

Mr. A. H. Simpson and Mr. H. D. Hufcut were elected active members.

JANUARY 5, 1886—THIRTIETH REGULAR MEETING.

William G. Stevenson, M.D., president, in the chair; two hundred fifty guests and members present.

Prof. Henry Van Ingen gave an address on "Ancient Sculpture," illustrated by lantern projections.

F. Monteser, Ph. D., was elected an active member.

FEBRUARY 2, 1886—THIRTY-FIRST REGULAR MEETING.

William G. Stevenson, M.D., president, in the chair; four hundred guests and members present.

D. B. Simmons, M.D., of Japan, gave an address on "The Social Status of Women in Japan."

In behalf of the Institute, the president extended to the speaker a cordial vote of thanks for his interesting address.

MARCH 2, 1886—THIRTY-SECOND REGULAR MEETING.

William G. Stevenson, M.D., president, in the chair; many members and a large number of guests present.

Prof. W. LeConte Stevens, of Brooklyn, N. Y., gave an address—illustrated with lantern projections—entitled "Aerial Navigation," of which the following is an abstract:

The power to rise above the ground was, among the ancients, regarded as exclusively a divine prerogative. This idea is illustrated in several mythical stories, such as that of Icarus and Daldalus, and the Persian legend of Kai Kaoos, in which disaster is shown to have followed every human attempt to exercise superhuman power.

Friar Bacon (1200–1300 A. D.) claimed the invention of a device for rising into the air, consisting of a thin globe of copper, "to be filled with ethereal air, or liquid fire," and then launched forth from some elevated point into the atmosphere. He shared in the popular idea that the atmospheric ocean covering the earth had a well-defined boundary like the aqueous ocean, and claimed to believe that his copper globe would float on the upper surface of the air as a ship floats on water. No account has been transmitted of any actual experiments by him on this subject, in the preparation of either liquid fire or a globe of suitable dimensions.

A Jesuit priest, Father Lana, about 1670, evolved independently Bacon's idea of using a copper globe, which he rightly supposed capable of being pushed up by the buoyant force of the surrounding atmosphere, if it could be made sufficiently thin and perfectly exhausted. He seems to have been aware of the previous experiments of Torricelli and Pascal, and proposed to exhaust his globe by filling it first with water, lifting it to a height exceeding thirty-four feet, attaching a controllable tube dipping into water below, and thus producing a Torricellian vacuum. He calculated the necessary diameter of his globe to be about twenty-five feet, and its thickness $\frac{1}{2}\frac{1}{2}\frac{1}{3}$ inch; but he had little conception

of the rigidity required to prevent it from breaking under the weight of the contained water if full, or collapsing under the pressure of the surrounding air if vacuous. His experiment was not actually tried, but the only valid objection to its probable success that he expressed was "that the Almighty would never allow an invention to succeed by means of which civil government could be so easily disturbed."

Several unsuccessful attempts were made about the same time to fly by the use of artificial wings, or to sail through the air by attaching to the body a frame-work covered with canvas, like an open umbrella. The futility of all attempts to fly was theoretically demonstrated by Borelli, an Italian physicist, whose posthumous work, *De Motu Animalium*, was published at Rome in 1680. His reasoning was based upon an examination of the muscular adaptation of birds to flight, and a consideration of the mechanical energy implied. He showed that the muscles and bony framework of the human body were far from adequate to do for it the same kind of lifting work that is accomplished by the bird, and hence that athletic training enough for purposes of flight was hopeless.

In 1766 Cavendish and Watt independently discovered the gas hydrogen, the most remarkable property of which was its low specific gravity. Dr. Black, of Edinburgh, attempted to utilize it for the purpose of causing a gas bag to ascend, but failed to secure a bag that was light enough. Cavallo, in 1782, caused soap bubbles to ascend by filling them with hydrogen. During the latter part of the same year, at Avignon, two paper manufacturers, the brothers Montgolfier, attempted similar experiments with hydrogen, using large paper bags as receptacles. They failed on account of inability to confine a gas of such high diffusive power within bags of such porous material as paper. Noticing that

clouds of vapor and smoke floated readily in the air, they conceived the idea of substituting these for hydrogen. Since the publication of Franklin's investigations on atmospheric electricity in 1752, the idea seemed to gain currency that electricity was an important agent in causing clouds to float in mid-air, and it was this that the Montgolfiers wished to utilize. A paper bag, with a capacity of about forty cubic feet, was held, the opening downward, over a fire of chopped straw and wool. It was quickly inflated and carried to the ceiling of the room. The experiment was soon repeated on a larger scale and a successful public exhibition was given at the village of Annonay on the fifth of June, 1783. The Montgolfiers seem to have regarded their success as due to the discovery of a new kind of gas, generated by the burning of straw and wool, rather that to the rarefaction of the air by heating it.

The news of the success at Annonay soon reached Paris, where a subscription was at once made up for the purpose of defraying the expense of constructing a balloon. The direction of the work was intrusted to Charles, a physicist, who had already become well known through his investigations in regard to the influence of temperature in modifying the density of gases. Charles at once comprehended the true principle underlying the success of the Montgolfiers, but decided to use hydrogen, confining it in a spherical bag, not of paper, but of thin silk varnished with india-rubber. The diameter of this balloon was twelve feet. If filled with pure hydrogen, its ascensive force would therefore have been a little over sixty pounds. On account of the defective method of preparing hydrogen at that time employed, much difficulty was experienced in inflating the balloon, but an ascent was successfully made on the afternoon of August 27, 1783, from the Champ de Mars, an open space on the outskirts of Paris, south of the river Seine. It remained

in the air about forty-five minutes, and was carried away fifteen miles by the wind, reaching a height of rather more than half a mile. In the experiment at Annonay, by the Montgolfiers, the inflation was accomplished in a few minutes, and the balloon remained in the air only fifteen or twenty minutes, the length of its voyage being limited by the rate of cooling of the contained hot air. On account of its quickness of action, the Montgolfier, or fire-balloon, was for some time more popular than the Charliére, or hydrogen balloon.

A few weeks after the experiment at the Champ de Mars, Stephen Montgolfier sent up a large fire-balloon at Versailles, in the presence of the king and nobility. A cage was attached to it, containing a sheep, a cock, and a duck. These, the first aeronauts, were carried to a distance of two miles, and reached the ground without special injury. The first ascent by a human being into the air, was made in a fire-balloon by Pilâtre de Rozier, a young French naturalist, who succeeded in partially controlling the ascent or descent of the balloon, by varying the amount of fuel with which the fire was kept supplied. He lost his life in 1785 by the explosion of a hydrogen balloon to which a fire-balloon was attached, his intention having been to secure steadiness by the use of hydrogen, and to vary the height by controlling the supply of fuel.

Having demonstrated the availability of hydrogen by his experiment on the twenty-seventh of August, Charles undertook the equipment of a new balloon, much larger than the first, for the purpose of ascending with instruments to examine the condition of the atmosphere at a great height. His balloon was provided with a safety-valve, and covered with a netting attached to a ring below, from which was suspended a car of wicker work. Ballast was provided, so that the weight to be lifted could be diminished at will

by throwing out sand, while the ascensive force could be controlled by means of the valve. This mode of varying the elevation of the balloon was far safer, and in every respect more convenient than that adopted with the fire balloon. An anchor was provided to help in making a successful descent on approaching the ground. Charles ascended to the height of nearly two miles on the first day of December, 1783, the barometer column sinking to 20.05 inches, and the thermometer falling from 41° F. to 21° F. Although this expedition was entirely successful, there is no record of his ever making another ascent. He was the real inventor of the balloon, and perfected it to such an extent that no very important improvement was made during the next two-thirds of a century.

It is rather remarkable that after the experiments just described, indicating the rapid development of aerostatics in France, the first ascent by a human being in any other country should be in America, so remote from France that the news of Montgolfier's experiment at Annonay did not reach here until six months afterward. The American astronomer, David Rittenhouse, at once conceived, independently of Charles, the idea of utilizing hydrogen for ballooning purposes. The Philadelphia newspapers of December 24 contained brief accounts just received in regard to Charles' experiment of August 27. In association with his friend, Francis Hopkinson, Rittenhouse first tried bladders filled with hydrogen, and then made forty-seven small balloons, which were filled with the same gas. No account has been transmitted of the mode of construction adopted. These were fastened around a cage in which animals were placed. This compound balloon was allowed to ascend, but held captive by means of a rope. A carpenter, James Wilcox, was then induced to ascend, and the rope was cut. He rose nearly a hundred feet, and descended by cutting incisions into several of the balloons.

Aside from the observations made by Charles during his first and only ascent, no use of the balloon was made for scientific purposes until after the beginning of the present century. In 1804, Gay Lussac ascended from Paris to a height of more than four miles, where he found clouds still overhead. He brought back specimens of air for analysis, which proved to consist of the same ingredients, in the same proportions, as that at the surface. In 1850, Bixio and Barral repeated Gay Lussac's experiment, and with the same result. They experienced sudden and unexpected changes of temperature, the thermometer falling from + 15° F., at twenty thousand feet, to — 38° F., at twenty-three thousand feet.

In 1861, a committee was appointed by the British Association, and an appropriation made to defray the expense of making a systematic examination of the upper atmosphere by the use of the balloon. The actual work of the committee was done chiefly by Mr. James Glaisher, who made twenty-eight ascents between 1861 and 1867. In that of September 5, 1862, he reached a height estimated to be thirty-seven thousand feet, incurring great peril in consequence of the extreme cold and the diminished density of the air. He made many observations of great value, which were duly published in his report to the Association.

During the early days of ballooning many futile attempts were made to propel and direct the aerostat. Indeed prior to 1852, balloon sailing was only aerial drifting, and not aerial navigation. Aerostatics was sufficiently understood, but aeronautics was still beyond human power. Meanwhile important changes had been made in ocean navigation as a result of the introduction of steam as a motive power for the driving, first of paddle wheels and afterward of screw propellers. In 1852, Giffard, a young French engineer, constructed an

elongated balloon (fig 1) which he inflated with coal gas. Suspended beneath it by cords was a longitudinal shaft, at the end of which was a triangular sail that could be turned about an axis and made to serve the purpose of a rudder. Below the shaft was a framework of wood supporting a small steam engine, whose piston gave motion to a screw propeller. He ascended with this to a height of five thousand feet and succeeded in making

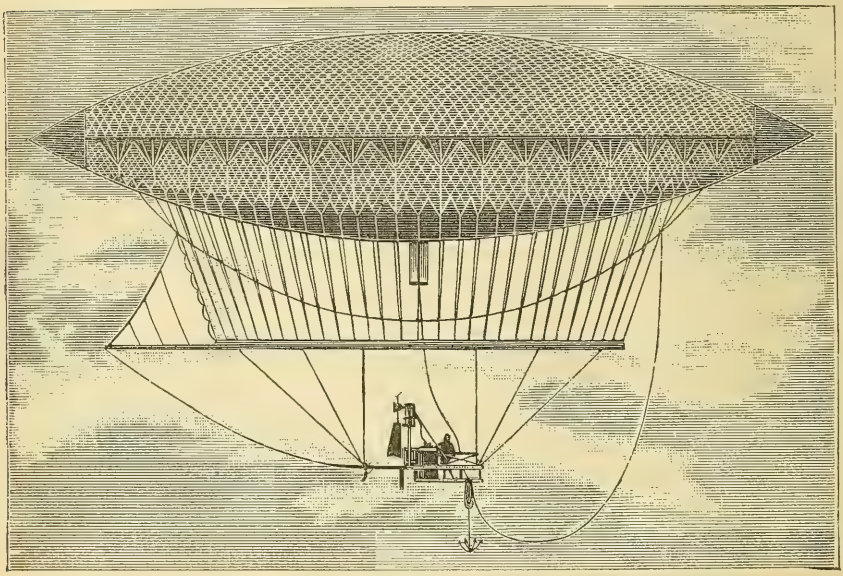

FIG. 1.

perceptible headway against a strong breeze, besides changing direction at will. Aerial navigation was thus proved to be possible, but this method was abandoned on account of the great danger, similar to that which had caused the death of Pilâtre de Rozier, and the difficulty in keeping the weight of the balloon constant. Giffard attained a speed estimated to be about nine miles an hour.

In 1872, Dupuy de Lôme constructed a balloon, much like that of Giffard but larger, and provided with a

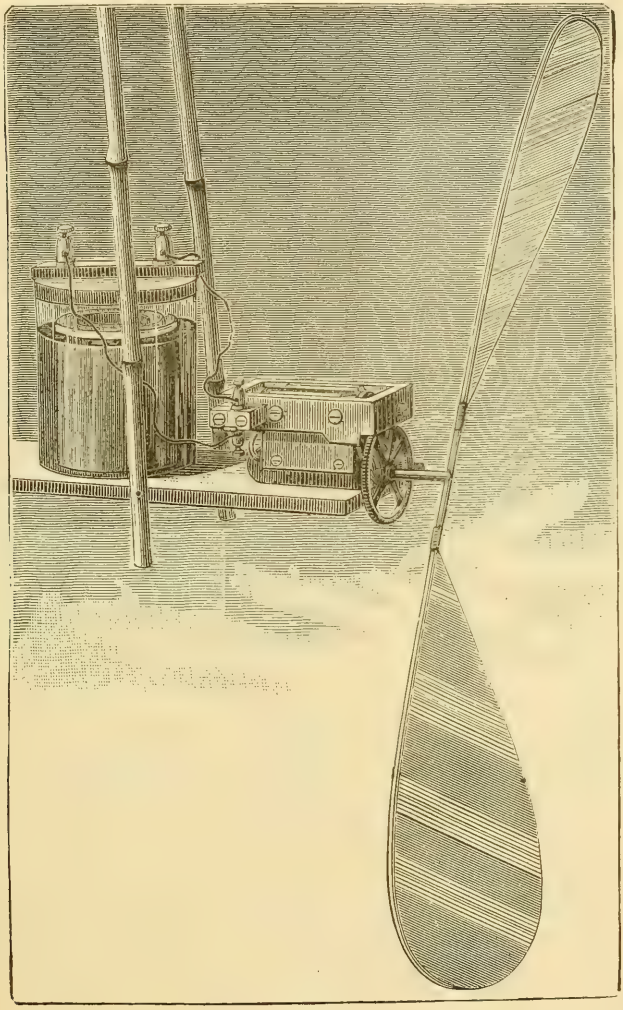

FIG. 2.

propeller which was to be controlled by hand rather than by steam power. He ascended with a force of four-

teen men and attained a speed estimated at six miles an hour. Muscular power was thus shown to be far too uneconomical to be employed in the direction of air ships.

In 1881, Tissandier, a pupil of Giffard, keeping abreast with the progress of electrical science, conceived the idea of employing storage batteries as the immediate source of energy to actuate the propeller of an elongated balloon. He constructed first a small experimental balloon which he inflated with hydrogen. Upon a light platform beneath it was placed a single storage cell, a Siemens motor, and a propeller (fig. 2, page 43). This was exhibited at the Electrical Exposition of 1881, when

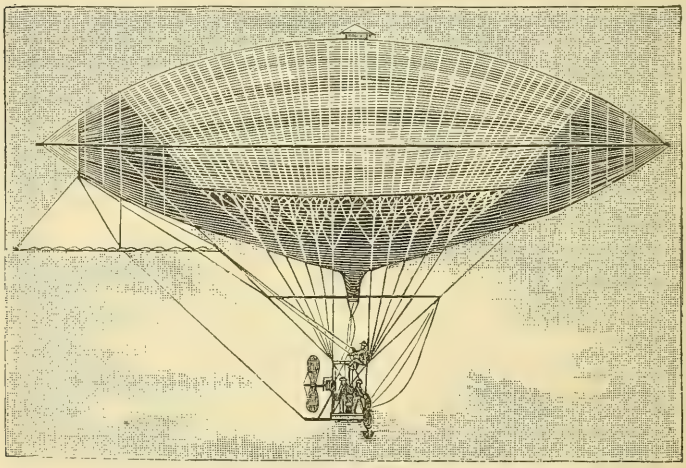

FIG. 3.

a speed of about ten feet per second was attained. He then undertook the preparation of a much larger balloon on the same plan (fig. 3), in which he ascended on the eighth of October, 1883. The motor weighed one hundred twenty-one pounds, and was excited by a battery of twenty-four cells, in series, weighing three hundred fifty pounds. Its effective capacity for work

was about one-and-a-half horse-power, equivalent therefore to about ten men, while its weight of battery and motor together was little over that of three men. He attained a speed estimated to be rather more than six miles an hour, but the sail rudder proved to be imperfect, and interfered with his experiment that was otherwise successful. A year afterward he made another ascent with this balloon and succeeded in attaining a speed about one-third greater.

Meanwhile MM. Renard and Krebs, officers of the French army, were conducting experiments at Chalais-Meudon, near Paris, on the conditions requisite for directing balloons, being guided in their studies by the previous work of Dupuy de Lôme. They constructed a balloon (fig. 4, page 46) one hundred sixty-six feet long, twenty-eight feet in its greatest diameter, with a capacity of sixty-seven thousand cubic feet and ascensional power of nearly five thousand pounds when inflated with hydrogen. The motive power was electricity, as with Tissandier's balloon. The propeller is fixed to the extremity of a long shaft and placed at the front, while the rudder, made of cloth, stretched tightly over a frame, is placed at the rear. In form it is symmetrical about a longitudinal axis, but the tapering is more abrupt at front than rear. The balloon is filled with hydrogen, but within it is a subsidiary balloon, connected by a tube with the cage below, where air can be pumped in or out at pleasure, thus varying slightly the specific gravity of the mass as a whole, and enabling the aeronauts to vary their elevation at will.

On August 9, 1884, Renard and Krebs ascended with this balloon, made a journey of nearly five miles, changing direction several times, and returning at the end of twenty-three minutes to the point of departure. On the eighth of November of the same year, they made two successful journeys, in the same afternoon, at-

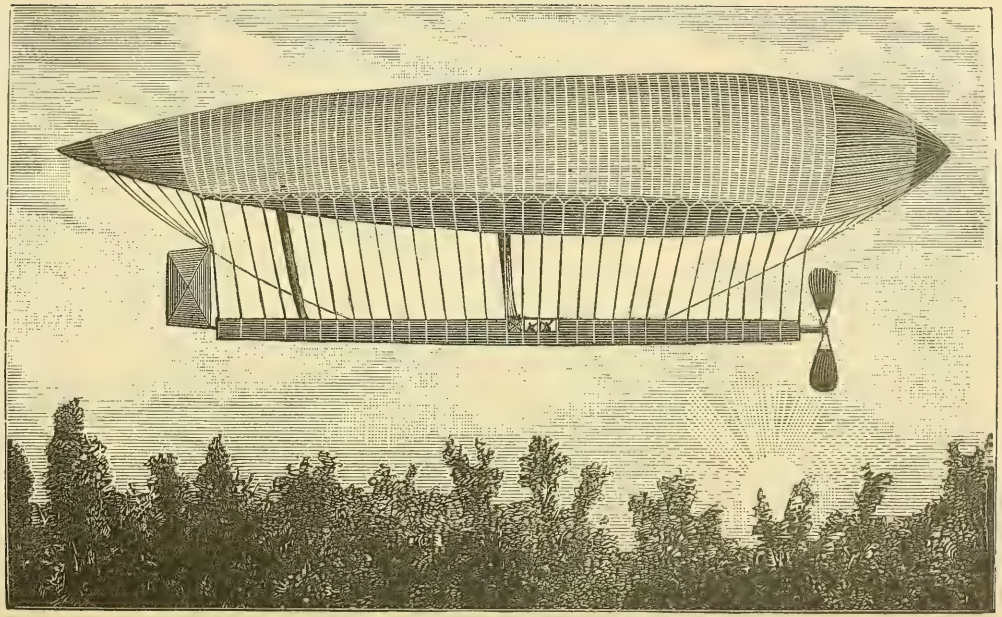

FIG. 4.

taining a speed estimated to be fifteen miles an hour, independently of the wind, which was blowing at the rate of five miles an hour. Similar journeys were again made on the twenty-second and twenty-third of September, 1885; in the last of which a speed was attained slightly in excess of that of the previous year.

The problem of aerial navigation, as a practicable and safe means of locomotion in favorable weather, has been definitely solved. But the great expense attendant upon both the construction and the manipulation of the balloon, and the limitation of its use to calm weather, excludes it from competition with other modes of transit currently in use. The aerostat, drifting with the wind or held captive with a rope, has already served an important purpose in time of war. The electric balloon will probably take its place, with the torpedo as a military appliance, too costly and uncertain for common use, but reserved for special occasions, when great exigencies require large expense and great risks.

A cordial vote of thanks was extended to Prof. Stevens for his interesting address.

APRIL 9, 1886—THIRTY-THIRD REGULAR MEETING.

William G. Stevenson, M.D., president, in the chair; many members and a large number of guests present.

Truman J. Backus, LL.D., of Brooklyn, N. Y., gave an interesting address entitled "The Battle of Long Island."

MAY 5, 1886—FIFTH ANNUAL MEETING.

William G. Stevenson, M. D., president, in the chair, twenty members present.

Capt. H. C. Taylor, U. S. N., and Mr. C. L. Flanders, were elected active members.

Dr. Stevenson, chairman of the committee on museum and library, gave an itemized report of expenditures for the museum and library, amounting to $131.25.

Prof. Cooley, chairman of the committee on publication, reported that five hundred copies of "Transactions" had been printed.

Mr. Elsworth, treasurer, rendered a detailed report of receipts and expenditures for the fiscal year ending May 4, 1886. The following is an abstract:

Balance in treasury, May 5, 1885,	$832 74
Total receipts during the year,	1,455 00
Total,	$2,287 74
Total disbursement during the year,	$1,478 45
Balance in treasury, May 4, 1886,	$809 29

Prof. Dwight, curator of the museum, presented the following report:

Your curator would respectfully make the following report for the session which is now closing:

The following additions have been made to the museum, by donation:

An unnamed collection of shells from Japan, presented by the Minister from Japan.

Two specimens of sphalerite in calcite, and two dozen geodes, containing quartz crystals, from Keokuk, Iowa, presented by Mr. Frank Fitchett, through Dr. Stevenson.

A few minerals and shells, from localities unknown, presented by Mrs. Ernest Yalden.

Two fossil shark's teeth, one fossil vertebra of the Zeuglodon whale, one "key-hole" urchin, presented by Dr. R. E. Van Gieson.

Some specimens of minerals from the Tilly-Foster mine, presented by Mr. Charles E. Fowler.

By exchange, from Dr. De Witt Webb in behalf of the Society of Natural Science, St. Augustine, Florida, through Dr. Stevenson, a large collection of shells, as follows:

One slab of shell-limestone, "Coquina-rock," St. Augustine, Fla.

One specimen *Fulgur perversa*, St. Augustine, Fla.

Two specimens *Fulgur carica* and its operculum, St. Augustine, Fla.
One specimen *Fulgur carica var. elisaeus*, St. Augustine, Fla.
Two specimens *Pinna semi-nuda*, St. Augustine, Fla.
Three specimens *Pinna muricata*, St. Augustine, Fla.
Five specimens *Cardium magnum*, St. Augustine, Fla.
Several specimens *Lucina edentula*, St. Augustine, Fla.
One specimen *Venus mortoni*, St. Augustine, Fla.
Four specimens *Venus mercenaria*, St. Augustine, Fla.
Six specimens *Arca pexata*, St. Augustine, Fla.
Several specimens *Arca incongrua*, St. Augustine, Fla.
Four specimens *Arca ponderosa*, St. Augustine, Fla.
Several specimens *Arca americana*, St. Augustine, Fla.
Several specimens *Labiosa (reta) canaliculata*, St. Augustine, Fla.
Two separate valves, *Labiosa lineata*, St. Augustine, Fla.
Two specimens *Ostraea virginica*, St. Augustine, Fla.
Several specimens *Dosinia discus*, St. Augustine, Fla.
One specimen *Pholas costata*, St. Augustine, Fla.
One specimen *Pholas tunicata*, St. Augustine, Fla.
Five specimens *Modiola plicatula*, St. Augustine, Fla.
Two specimens *Solecurtus gibba*, St. Augustine, Fla.
Two specimens *Lucina pennsylvanica*, St. Augustine, Fla.
Two specimens *Mactra semelis*, St. Augustine, Fla.
Two specimens *Iphigenia, sp. (?)*, Mosquito Inlet, Fla.
Several specimens *Tellina alternata*, St. Augustine, Fla.
One specimen *Tellina interrupta*, West Indies.
Three specimens *Tellina radiata*, West Indies.
Three specimens *Tellina, sp. (?)*, West Indies.
Six specimens *Crepidula fornicata*, St. Augustine, Fla.
Several specimens *Crepidula convexa*, St. Augustine, Fla.
Four specimens *Crepidula unguiformis*, St. Augustine, Fla.
Five specimens *Crepidula glaucus*, St. Augustine, Fla.
Two specimens *Crepidula aculeata*, St. Augustine, Fla.
Fifteen specimens *Fissurella alternata*, St. Augustine, Fla.
Three specimens *Fissurella nodosa*, St. Augustine, Fla.
Three specimens *Fissurella sp. (?)*, St. Augustine, Fla.
Twenty-six specimens *Siphonaria alternata*, St. Augustine, Fla.
Many specimens *Donax variabilis*, St. Augustine, Fla.
Four specimens *Sigaretus perspectivum*, St. Augustine, Fla.
Four specimens *Natica mammalaris*, St. Augustine, Fla.
Nine specimens *Nerita peleronta*, West Indies.
Ten specimens *Nerita tesselata*, West Indies.
Nine specimens *Nerita sp. (?)*, West Indies.
Two specimens *Nerita sp. (?)*, West Indies.
Nine specimens *Neritina reclivata*, St. Augustine, Fla.
Several specimens *Littorina irrorata*, St. Augustine, Fla.

Thirteen specimens *Littorina augulifera*, St. Augustine, Fla.
Eight specimens *Littorina sp.* (*?*), St. Augustine, Fla.
Four specimens *Trochus sp.* (*?*), St. Augustine, Fla.
Three specimens *Tectarius nodosus*, West Indies.
Six specimens *Tectarius muricata*, West Indies.
Several specimens *Nassa vibex*, St. Augustine, Fla.
Eight specimens *Nassa trivittata*, St. Augustine, Fla.
Several specimens *Nassa obsoleta*, St. Augustine, Fla.
Six specimens *Nassa sp.* (*?*) West Indies.
Many specimens *Urosalpinx cinerea*, St. Augustine, Fla.
Seven specimens *Terebra dislocata*, St. Augustine, Fla.
Three specimens *Terebra sp.* (*?*), West Indies.
Eleven specimens *Cerithium sp.* (*?*), West Indies.
Four specimens *Cerithium sp.* (*?*), West Indies.
Several specimens *Cerithium eburneum*, St. Augustine, Fla.
Five specimens *Cerithium sp.* (*?*), St. Augustine, Fla.
Several specimens *Cerithidea scalariformis*, St. Augustine, Fla.
Several specimens *Columbella avara*, St. Augustine, Fla.
Several specimens *Columbella mercatoria*, St. Augustine, Fla.
Three specimens *Eupleura caudata*, St. Augustine, Fla.
Five specimens *Scalaria lineata*, St. Augustine, Fla.
Four specimens *Scalaria humphreysiana*, St. Augustine, Fla.
Several specimens *Volva uniplicata*, St. Augustine, Fla.
Two specimens *Pyramidella dolabrata*, West Indies.
Several specimens *Olivella mutica*, St. Augustine, Fla.
Eleven specimens *Oliva literata*, St. Augustine, Fla.
Two specimens *Oliva inflata*. St. Augustine, Fla.
Six specimens *Marginella, sp* (*?*), South Florida.
Seven specimens *Marginella, sp* (*?*).
Several specimens *Melampus tridentatus*, St. Augustine, Fla.
Six specimens *Cyprœa moneta*, St. Augustine, Fla.
Four specimens *Cyprœa helvola*.
Two specimens *Cyprœa lynx*.
Twelve specimens *Trivia pedicularis*, West Indies.
Eight specimens *Purpura floridana*, St. Augustine, Fla.
Two specimens *Fasciolaria distans*, St. Augustine, Fla.
One specimen *Fasciolaria tulipa*, St. Augustine, Fla.
Five specimens *Bulla sp.* (*?*), West Indies.
Five specimens *Vermetus lumbricalis*, Nassau, N. P.
Many specimens *Odostomia impressa*, St. Augustine Fla.
Two specimens *Strombus gigas*, Lake Worth, South Florida.
Two specimens *Strombus fragilis*, Lake Worth, South Florida.
Three specimens *Planaxis sp.* (*?*), West Indies.
Several specimens *Terebra dislocata* (or *rudis ?*),

One specimen *Lithodomus appendiculata*, buried in coquina rock, St. Augustine, Fla.
Several specimens *Unio fuscata*, tributary of St. John's River, fifteen miles west of St. Augustine, Fla.
Several specimens *Unio jayensis*, Ocklawaha River, Fla.
One specimen *Unio dariensis*, Cowan Swamp, near St. Augustine, Fla.
Two specimens *Unio sp.* (*?*), Crescent Lake, Fla.
Several specimens *Mytilus cubitus*, St. Augustine, Fla.
Five specimens *Spirula peronii*, *loc* (*?*).
Many specimens *Helix carpentariana*, St. Augustine, Fla.
Thirteen specimens *Helix septemvalva*, St. Augustine, Fla.
Fifteen specimens *Helix cereolus*, St. Augustine, Fla.
Three specimens *Helix auriculata* (*?*), St. Augustine, Fla.
Twenty specimens *Helix orbiculata*, St. Augustine, Fla.
Eight specimens *Planorbis antrorsus*, St. Augustine, Fla.
Six specimens *Ampullaria depressa*, Bike's Hammock, thirty miles south of St. Augustine, Fla.
Seven specimens *Ampullaria hopoensis*, Durbin Creek, tributary of St. John's River, eighteen miles northwest of St. Augustine, Fla.
Twelve specimens *Paludina wareana*, Durbin Creek, Fla.
Four specimens *Paludina georgina*, Lake Jessup, Fla.
Many specimens *Succinea campestris*, St. Augustine, Fla.
Two specimens *Strophia uva*, West Indies.
Several specimens *Pecten dislocata*, St. Augustine, Fla.
Several unnamed gasteropod shells, St. Augustine, Fla.

By purchase from Mr. William Colby ; ninety-three specimens, as follows :

Two specimens *Nyctiardea grisea-nœva*, night heron.
Two specimens *Limosa fœda*, great marbled godwit.
One specimen *Sterna hirundo*, Wilson's tern.
Two specimens *Harelda glacialis*, long-tailed duck.
One specimen *Querquedula carolinensis*, Am. green-wing teal.
One specimen *Ægialites melodus*, piping plover.
Two specimens *Colaptes auratus*, golden-wing woodpecker.
One specimen *Melanerpes erythrocephalus*, red-headed woodpecker.
Two specimens *Larus* (*argentatus*) *smithsonianus*, Am. herring gull.
One specimen *Accipiter cooperi*, Cooper's hawk.
Five specimens *Accipiter fuscus*, sharp-shinned hawk.
Four specimens *Falco columbarius*, pigeon hawk.
One specimen *Totanus melanoleucus*, greater tell-tale.
One specimen *Tringoides macularius*, spotted sand-piper.
Two specimens *Buteo borealis*, red-shouldered buzzard.
One specimen *Alle nigricans*, sea dove.
Two specimens *Sphyropicus varius*, yellow-bellied woodpecker.

Two specimens *Tyrannus carolinensis*, king bird.
Three specimens *Chordediles popetue*, night hawk.
One specimen *Coccygus americanus*, yellow-bellied cuckoo.
One specimen *Ceryle alcyon*, belted king-fisher.
Two specimens *Quiscalus purpureus*, purple grackle.
Three specimens *Scolecophagus ferrugineus*, rusty grackle.
One specimen *Molothrus ater*, cow bird.
One specimen *Sturnella magna*, meadow lark.
Three specimens *Icterus spurius*, orchard oriole.
Two specimens *Carpodacus purpureus*, purple finch.
One specimen *Astragalinus tristis*, yellow bird.
One specimen *Plectrophanes nivalis*, snow bunting.
One specimen *Passerella iliaca*, fox sparrow.
One specimen *Pipilo erythrophthalmus*, ground robin.
One specimen *Vireo olivaceus*, red-eyed greenlet.
Three specimens *Ampelis cedrorum*, cedar bird.
One specimen *Zonotrichia albicollis*, white-throated sparrow.
One specimen *Pyranga rubra*, scarlet tanager.
One specimen *Dendræca æstiva*, golden warbler.
One specimen *Parula americana*, blue yellow-back warbler.
One specimen *Sialia sialis*, blue-bird.
One specimen *Turdus migratorius*, robin.
One specimen *Turdus mustelinus*, wood-thrush.
One specimen *Mimus carolinensis*, cat-bird.
Three specimens domestic pigeons.
One specimen *Phasianus colchicus (?)*, silver pheasant.
One specimen *Mimus polyglottus*, mocking-bird.
Two specimens *Dendræca coronata*, yellow-rumped warbler.
One specimen *Psittacus sp. (?)*, parrot.
Twelve specimens as yet unnamed.
One specimen *Tamias striatus*, ground-squirrel.
One specimen *Alligator mississippiensis*, alligator.
One specimen *Mus decumanus*, brown and white rat.
Two specimens *Rana catesbiana*, bull-frog.
One pair of jaws of *Carcharias obscurus*, blue shark.

In the museum the zoological specimens have been thoroughly examined and cleaned; some of the cases which were purchased from the late Poughkeepsie Society of Natural Science, have been altered and renovated. The valuable collections of plants, donated at various times by Mr. Gerard, Dr. Van Gieson, Dr. Stevenson, and Prof. Hyatt, have urgently needed remounting and careful arrangement and classification.

For this purpose a large amount of suitable paper and genus covers of excellent quality have been purchased, and the laborious task of transferring and labelling these specimens and providing them with suitable shelves is now in process of accomplishment.

Almost the entire work in the museum above mentioned has been performed under the active superintendence and constant personal attention of the assistant curator, Dr. Stevenson. The Society is also much indebted to Mr. Gilbert Van Ingen for valuable assistance in the arrangement and classification of the botanical specimens.

Respectfully submitted,

WM. B. DWIGHT,
Curator.

Mr. Elting, librarian, reported that fifteen bound volumes and one hundred thirty-two pamphlets (mostly the transactions of various scientific societies) had been received as donations to the library.

The Key of North American Birds (Coues),
Structural and Systematic Conchology (Tryon),
Text-book of Zoölogy (Claus and Sedgwick),
The Determination of Rock-forming Minerals (Hussack)

have been added, by purchase, by the committee on museum and library.

Mr. Bristol, secretary, presented the following annual report:

To Vassar Brothers Institute: Your secretary, in accordance with his prescribed duties, has the honor to submit to you his annual report as follows:

One member, Mr. James Smillie, has died during the year, and five active members have been elected.

The following addresses were delivered before the Institute:

1885.

November 10. "Interpretations of Nature."
Wm. G. Stevenson, M.D., president of the Institute.

December 1. "The Quakers and Their Doctrines."
James M. DeGarmo, Ph.D.

1886.

January 5. "Ancient Sculpture."....Prof. Henry Van Ingen.

February 2. "The Social Status of Women in Japan."
D. B. Simmons, M.D.

March 2. "Aerial Navigation."....Prof. W. LeConte Stevens.

April 9. "The Battle of Long Island."..Truman J. Backus, LL.D.

Volume III of the Transactions of the Institute and its Scientific Section, containing two hundred sixteen pages, has been issued, and distributed among the various scientific and philosophical societies with which we exchange. The following papers are contained therein:

"The Evolution of Science."....LeRoy C. Cooley, Ph.D.
"The Just Claims of Natural Science."....Prof. W. B. Dwight.
"The Genealogy of the Vertebrata, as learned from Paleontology." } Prof. E. D. Cope.
"Some changes in the Habits of Birds."....James M. De Garmo, Ph.D.
"Note on the Downy Woodpecker."....Mr. George Tremper.
"Sand and Kaolin from Decaying Quartzite."....Prof. W. B. Dwight.
"The Jolly Balance."....LeRoy C. Cooley, Ph.D.
"The Peculiar Structure of Clark's Clay Beds."...Prof. W. B. Dwight.
"An Empirical Study of Gyrating Bodies."....C. B. Warring, Ph.D.
"Recent Celestial Phenomena."....Prof. Maria Mitchell.
"Sun Spots."....Miss Mary W. Whitney.
Chairman's Annual Report....Prof. W. B. Dwight.
Van Duzer and Townsend Mines....Corrigenda.

We have received contributions for our library during the past year from the following sources, viz:

Anthropological Society, Washington, D. C.
American Philosophical Society, Philadelphia.
Albany Institute, Albany, N. Y.
American Geographical Society, New York.
American Monthly Microscopical Journal.
Academy of Sciences of St. Louis, Mo.
Academy of Natural Sciences of Philadelphia.
American Museum of Natural History, New York.
Boston Society of Natural History, Boston, Mass.

Buffalo Society of Natural Science, Buffalo, N. Y.
Bureau of Education, (Department of the Interior).
Bureau of Ethnology, (Department of the Interior).
Bureau of U. S. Geological Survey, (Department of the Interior).
California Academy of Sciences, San Francisco, Cal.
Cornell University, Ithaca, N. Y.
Connecticut Academy of Arts and Sciences, New Haven, Conn.
Cincinnati Society of Natural Science, Cincinnati, Ohio.
Canadian Record and Natural History Society of Montreal.
Department of the Interior.
Engineer Department of U. S. Army.
Library Company, Philadelphia.
Minnesota Academy of Natural Sciences, Minn.
Missouri Historical Society, St. Louis, Mo.
New York Academy of Sciences, New York.
Natural History Society of Glasgow, Scotland.
Nova Scotian Institute of Natural Science, Halifax.
Newport Natural History Society, Newport, R. I.
New York Microscopical Society, New York.
Oberhessischen Gesellaschaft fur Natur-und Heilkunde, Giessen.
Psyche.
Portland Society of Natural History, Portland, Me.
Rochester Society of Natural Science, Rochester, N. Y.
Royal Society of Canada.
Sociedad Cientifica Argentina, Buenos Aires.
Société Impériale des Naturalistes de Moscou, Russia.
Smithsonian Institution.
Torrey Botanical Club, New York.
U. S. Fish Commission, Washington, D. C.
Wisconsin Natural History Society.
Wisconsin Academy of Sciences, Madison, Wis.

The Scientific Section held the following meetings:

1885.

November 18. "Cosmogony of the Quichi Indians."
C. B. Warring, Ph.D.
December 2. "Geodes." .....Prof. W. B. Dwight.
December 16. "The Periodic Law of the Elements."
L. C. Cooley, Ph.D.

1886.

January 13. "The Axioms of Geometry."F. Monteser, Ph.D
January 13. "Decomposition of the Element of Didymium."
L. C. Cooley, Ph.D.

January 27. "Primordial Rocks of the Wappinger's Limestone."
Prof. Wm. B. Dwight.
February 10. "Review of the Controversy between Mr. Gladstone and Prof. Huxley relating to Genesis and Science."
C. B. Warring, Ph.D.
February 24. "Supplementary Paper on the Top."
C. B. Warring, Ph.D.
April 10. "The Isthmian Canal.." Capt. Henry C. Taylor, U.S.N.

At the annual meeting of this Section, held May 4, Dr. Stevenson was elected chairman, and Mr. C. N. Arnold was re-elected recording secretary.

The following essays and discussions were given before the Literary Section:

1885.
November 17. "The Attitude of Political Parties towards the Temperance Question."....Mr. C. B. Herrick.
November 24. "Popular Prejudices."....Mr. Edward Burgess.
December 8. "Gladstone: His Political Leadership and its Effects."
Mr. Cyrus Swan.
December 15. "Poems of George Eliot."....Mr. Henry V. Pelton.
"Poems of Ralph Waldo Emerson."
Mr. Irving Elting.

1886.
January 12. "Prof. Sumner's Free Trade Theory."
Rev. Wayland Spaulding.
January 19. "Bismarck: His Leadership and its Effects."
Mr. Edward Burgess.
January 26. "The Jews in History."....Mr. L. F. Gardner.
February 9. "Robert Burns."....Mr. J. Hervey Cook.
February 16. "The New Theology."....Rev. Howard B. Grose.
February 23. "Waterloo and Napoleon Bonaparte."
Mr. Edward Elsworth.

At the annual meeting of this Section Mr. L. F. Gardner was elected chairman, and Mr. J. H. Hamill was re-elected recording secretary.

Respectfully submitted.

C. L. Bristol,
Secretary.

The following gentlemen were elected trustees of the Institute for 1886–1887 :

John Guy Vassar,	William G. Stevenson,
S. M. Buckingham,	Edward Elsworth,
Joachim Elmendorf,	LeRoy C. Cooley,
William B. Dwight,	Henry V. Pelton,
Charles B. Herrick,	A. P. Van Gieson,
William T. Reynolds,	Charles N. Arnold.

The following gentlemen were elected officers of the Institute for 1886–1887 :

President, . . .	William G. Stevenson, M.D.
Vice President, .	Mr. Henry V. Pelton.
Secretary, . . .	Mr. Charles L. Bristol.
Treasurer, . . .	Mr. Edward Elsworth.
Curator of Museum,	Prof. William B. Dwight.
Librarian, . . .	Mr. Irving Elting.
Art Director, . .	Prof. Henry Van Ingen.

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

1886–1887.

NOVEMBER 9, 1886—THIRTY-FOURTH REGULAR MEETING.

Two hundred twenty-five members and guests present.

ANNUAL ADDRESS.

GENIUS AND MENTAL DISEASE.

BY WILLIAM G. STEVENSON, M.D., PRESIDENT OF THE INSTITUTE.

Members of the Institute, Ladies and Gentlemen: As an expression of energy the human mind is the highest; as a product of creative power through organic evolution it is supreme, and as a mystery in nature it is the most profound. It were comparatively an easy task to explain psychological phenomena by asserting, as did the metaphysicians of the past, and as some do even at the present, that the human brain—the physical sanctuary of thought—is merely an instrument through which various spiritual beings operate, producing at one time the prophetic utterances of the seer, at another time the gifted words of genius, and yet again the extravagant and discordant expressions of madness. This was the "working hypothesis" of Pagan antiquity in its efforts to explain the utterances of its oracles, and also of the

Christian fathers in their attempts to explain the inspiration of the prophets and of the apostles.

Greek supernaturalism and the Christian doctrine of inspiration here found a common point of agreement, for both implied a "divine intoxication"—an "overflowing of the mind"—because of its entire possession by a divine influence, which, according as it was good or evil, excited a "poetic furor" indicative of genius, or caused a wild frenzy which was known as madness.

Genius, therefore, was simply a reflection, through the human brain, of an outside divinity of good; while insanity was merely an expression of satanic possession—an inspiration of an evil spirit—and in nature was closely allied to genius.

This belief, although somewhat modified by filtering through ages of changing thought, has been superseded only in very recent times by the conceptions which reflect the broader generalizations of inductive science.

Modern science, groping its way through the intricacies of inanimate and animate nature, has applied the inductive method to the study of mind; made the brain the starting point of its analysis, and, from a study of its mechanism, sought to learn its relations to psychical phenomena. For centuries this has been the inspiring hope of many; but the ultimate facts of nervous tissue yet elude us. The fragments of truth already obtained have, however, widened the horizon of our mental perspective, and caused to recede—yet a little further, and on a different tangent—the unknown which we pursue.

Every discovery has opened new avenues of thought, and suggested new problems for solution, and, at the same time, there has come to the thoughtful mind a deeper realization of nature's vastness, and of the profound mystery that lies behind the phenomena which appear to the senses and are perceived by the intellect.

There can be no complete synthesis of mental phe-

nomena until analysis has furnished all the facts which pertain to the "physics of the brain"; and since the spirit of modern science seeks only truth, regarding all theories and hypotheses merely as provisional instruments to this end—to be discarded for better ones whenever a newly-discovered fact shall indicate the need of change—it is important to examine from time to time the record of verified facts, that we may more justly estimate the value of any theory pertaining either to matter or to mind.

In the study of the brain, anatomy has revealed its complicated structure, and traced its development through many forms of life upward to man; physiology and pathology have studied the wondrous functions of its various parts, and witnessed the psychological changes which follow its growth and decay; chemistry has largely solved the riddle of its elements, and given us the agents which, without altering the most delicate nerve-substance, fix it in all its natural relations and so fit it for detailed examination; while the microscope—peering into the secrets of its cell formations—has revealed myriads of elements unknown to the investigators of the past, and caught a shadowy outline of its molecular activities.

From the data thus furnished comes the conviction that mental phenomena "are dependent upon the properties and molecular activities of nerve-tissue"; and that there is a "bond of union" between psychical expressions and a nervous mechanism, although the nature of this union is unknown. The facts of consciousness are marshaled before us with all the force of attested verities, but are yet veiled with all the mystery of a passing dream.

If, then, neither science nor philosophy has fully classified mental phenomena as observed in the general average of mankind, how vain will be the attempt to

measure the intellectual greatness of genius or tell the story which belongs to the degeneration of the reasoning mind!

That there may be a close relationship between genius and madness is a proposition which impresses us, at first, as a violation of the order of nature; but when we think that the unity of nature includes in its cycle not the world of matter alone, but living things and mind—wherein are wrought marked differences of form and expression through gradations so imperceptible that we do not discern the real points of transition—and reflect, further, upon the changing forms of certain nervous diseases and morbid predispositions in their "pathological evolution through generations," and recall the fact that some originating minds have been touched with insanity or, perhaps, it might be said, in some cases, that so much of madness has been accompanied with expressions of such intellectual greatness, the question of kinship between genius and madness becomes one of deep physiological and psychological interest.

The quality of mind known as genius involves, in connection with the reasoning faculties, the special exercise of imagination in its higher creative or constructive forms; and in understanding this faculty we have an insight into the marvelous nature of genius.

It may be said that imagination is that faculty which, in its lower or constructive form, works within the limits of recollection, and transforms the materials of sense-experience into pictures of thought, and recombines them into forms of greater beauty and usefulness; while in its higher or creative form it distills therefrom truths which reason has not yet discerned, and idealizes beauties and excellences which excite our admiration and exalt our emotions.

When thought symbolizes to the mind "the forms of things unknown," it is because the imagination—leap-

ing beyond the bounds of sensory perception—gathers from the infinitudes of unrevealed realities new truths, and thereby "gives to airy nothing, a local habitation and a name." It is thus that the intellect is able to extend the horizon of knowledge, and obtain material for the workshops of the brain.

Imagination is the prolific mother of art, of poetry, and song; its breath transforms dull canvas into a thing of beauty, and gives to marble the expressive forms of life. The drama, the poem, the symphony are the grand chorus of its inspiration. The domination of law throughout the universe, and the unity of nature under it, are its revelations, while the hypotheses of science and the postulates of philosophy are but its temporary vestures.

Imagination, however bold may be its flight, is, nevertheless, under the restraining influence of reason, and performs its wondrous work along true parallels of thought. Its ideals are not mere symbols of myths and fleeting shadows, but ideals which are the embodiments of eternal truths. Thus, by its sovereignty in realms where Ariadne's thread is lost from view, the imagination constructs its empire, and gives by its own methods new revelations of truth, thereby "converting all nature into the rhetoric of thought."

This, then, is the special mind-quality—the "vision and the faculty divine"—which constitutes the power of genius.

Notwithstanding its high endowments, genius is not so puissant that it cannot be "improved by culture, directed by knowledge or disciplined by reason"; nor so transcendent that it "knows without learning, and teaches the world what it never learned." To say with Mr. Stedman that "genius lies in the doing of one thing, or many things, through power resulting from the unconscious action of the free intellect in a manner unattainable by the conscious effort of ordinary men" is

much the same as to say—what every one knows—that the unconscious or reflex action of some highly endowed minds, evolves intellectual products—of which we are conscious—which outrank the effects emanating from the conscious effort of less gifted minds.

Yet what avails either conscious or unconscious mentality unless it utilizes the rich legacies of the past, and, to the experience, knowledge, and culture of other minds, adds that which will enlarge our conceptions of truth and beauty. Herein is genius—that conscious or unconscious "mastery which," as Mr. Howells says, "comes to any man according to his powers and diligence in any direction." This does not affirm equality of all minds, nor allege that diligence or discipline can enable all to reach the same level of intellectuality, but it implies that the intellect is a unity, and that its varied expressions—whether known as "ability," "talent" or "genius"—differ not in kind but in degree.

Genius, as the superlative expression of imaginative thought, often acts in an orbit of great eccentricity, and may be accompanied with a temperament so keenly sensitive that its tides of emotional life are ever at the highest flood or at the lowest ebb. "It is not happiness, but ecstasy; not grief, but anguish; hope becomes enthusiasm, and despondency leads to despair."

In this glow of thought—this fierce consuming fervor of emotional life—it were not strange if the mind-tension, thus intensified by the restless energy of "spiritual generation," should at times "disturb the harmony of cerebral action and thereby weaken volitional control."

In the attempt, not to define genius, but to explain the order of its succession, Mr. Galton was led to "conclude that each generation has enormous power over the natural gifts of those that follow," and that native endowments of mind are of themselves quite sufficient to

enable an individual to become "eminent" or even "illustrious."

Heredity is, therefore, the silent conservator and directing factor of mental life; it is that inborn power of organization which enables "one out of a million" to become a genius, and to surmount all obstacles and neutralize every repressive influence, while others who have not this exalted birthright fall by the way and lose their individuality in the vast throng of a common humanity.

That there is a profound principle of truth involved in the question of heredity cannot be denied, and that the factor of inheritance is the most essential of any which enters into the complex equation of mind as well as of body, is a well-established fact; but it is not the only factor which determines mental expression, nor can a complete classification of known facts be made from it alone. Heredity explains the existence of a general nervous constitution, a brain-fiber, having definite aptitudes or "organic dispositions," which are transmitted from parent to offspring, securing thereby not only a continuity, but a conservation of psychical as well as of physical properties; but the special way in which this mental aptitude shall show itself is largely dependent upon external influences or an unexplained spontaneity.

Organization limits the influence exerted by environment, while environment limits and modifies the development of the capacities of the organization.

The explanation of genius through the operation of the biologic law of heredity is very satisfactory so long as antecedent and sequence bear to each other definite and ascertainable relations; but trouble begins when the genetic record fails in its apparent unity—as when genius and mediocrity have kinship.

Whence came the genius of Phidias, which enabled him with such immortal art to create in carved ivory and

fretted gold the Lemnian statue of the Parthenon and the Zeus of Olympia ; whence came the power of Michael Angelo, Salvator Rosa, Leonardo da Vinci, and Rubens, to paint in matchless beauty, on canvas and in fresco, the wondrous imagery of their minds ; or of Beethoven, to record in his symphonies the raptures of his soul ; or of Scott, to clothe with the habiliments of life the ideals of his brain ; or of Spenser, Burns, and Byron, to write with such rhythmic beauty ; or of Goethe, to garnish with poetic dress the deep philosophy of his thought ? In what cloudland of the past were hidden the possibilities of Dante and Milton—who made their visions of the eternal realms the subject of impassioned verse—at once gorgeous in its rich tracery of thought, and sublime in its pageantry of bliss and woe ? In what ancestral brain did sleep the transcendent genius of Shakespeare that read every page "in nature's infinite book of secrecy"; or did smolder the giant intellect of Newton, which weighed the planets and bound with the force of gravity atoms and worlds in a bond of unity ?

Such examples seem indicative of conditions powerful to modify, transform, or deflect the action of the laws of heredity, and to cause "indefinite variability" in psychological phenomena, as is done in material forms. This variability, this new psychic manifestation, is robed with the insignia of a new creation ; a new species has been born into the realm of mind, displaying new and more exalted powers, but nevertheless restrained in its action by the organization which, under law, presides with such tyranny over every mental expression, and makes us, to a greater extent than we commonly think, creatures of an inexorable destiny. The contrast between the exalted ideals and grand achievements of genius, and the feeble, discordant expressions of madness, is as pathetic as it is striking. The citadel of

thought has been despoiled of its most precious adornment, and in the place where once the Muses sat, mocking echoes now hold carnival, and "melancholy sits on brood."

To give a definition of insanity which shall prove acceptable to medical psychology and to practical jurisprudence, is a more difficult task than it may appear. This comes because of the differences in the appreciation of causes and effects in mental phenomena which exist between minds trained in the technical details of physiological and pathological knowledge, and those who witness merely a few of the more pronounced expressions of lunacy, but are unable to trace the expressions to their relating causes.

To have even a moderate understanding of insanity, it is necessary to clearly comprehend the nature and import of "illusion," "hallucination," and "delusion"—which, when they exist, are of so much importance that some would fain have us believe that the possession of any one of these symptoms is sufficient to make genius and insanity "a little more than kin, and less than kind."

When a person sees, hears, smells, tastes, or feels an object, but perceives it to be what it is not—as when a tree becomes a man, and the murmuring wind his voice —an illusion exists ; a real sense-impression is wrongly interpreted by the perceptive centers, and hence the perception does not correspond with the external object.

Hallucination originates within the brain, and is the perception of that which has no real existence ; indeed, so purely subjective is it, that the senses have no agency in its production. Under conditions of concentrated attention, ideas, feelings, and sense-perceptions, are marshaled into consciousness with as great distinctness as if they were the products of external objects, rather than of subjective conditions alone. This comes

from the fact that the sense-centers are influenced by impressions received independent of their source. Their function is to transform impressions into conscious sensation, and hence an idea or emotion, when directed in a special way with persistent, concentrated force, may so impress the sensorium as to cause it to project into consciousness sensations which seem to come from objects in the external world. I cannot tell how this is done, neither can I tell how it is done when impressions come from without. The facts we know, but the secrets of transformation elude us. The brain constructs new forms, but conceals the methods of imagination by the shadow of unconsciousness.

Ajax becomes enraged because the arms of Achilles are given to Ulysses, and in his wrath he sees animals as Greeks and assails them as if Ulysses and Agamemnon themselves were before him. Talma intensified his emotions and his dramatic effect by the illusive specters of his mind. Spinoza beheld with great distinctness the disagreeable image of his dream a long time after sleep was gone; and Niebuhr, when describing the scenes of his travels, would see all rise before him in "all the coloring, animation, and splendor of nature."

Multitudes have been at times subject to the same false perceptions; as when the soldiers under Constantine saw the cross in the sky bearing the inscribed words, "*In hoc signo vinces*"; or when the army at the battle of Antioch, excited and superstitious, saw the saints—George, Demetrius, and Theodosius—descending through the clouds of heaven to their support.

The consummate skill of Shakespeare in portraying the different phases of false perception, and his power of psychological analysis, are wonderfully illustrated in the dagger-scene of *Macbeth*. Intent on murder, with "courage screwed to the sticking-place," Macbeth is about to enter the king's chamber, when he is startled

and dismayed by an apparition of a bloody dagger in the air. For a moment he questions the reliability of his sight, and exclaims :

> "Is this a dagger which I see before me,
> The handle towards my hand?"

He cannot believe the testimony of his eyes, and therefore seeks confirmation in the sense of touch :

> ". . . Come, let me clutch thee :
> I have thee not, and yet I see thee still."

Failing to grasp the dagger, he wonderingly asks :

> "Art thou not, fated vision, sensible
> To feeling as to sight?"

And then, as if reason were struggling to gain supremacy over the senses, he continues :

> ". . . or art thou but
> A dagger of the mind, a false creation
> Proceeding from a heat-oppressed brain?"

How suggestive, how replete with truth was this prophetic utterance; and yet the intensity of his mind's tension—because of the deed to be done and the instrument for its execution—still makes the terrible idea the dominating factor of his mind, and subordinates the senses to its rule! He is not yet able to entirely dispel the hallucination, and he compares the apparition to the trusted blade at his side :

> "I see thee yet, in form as palpable
> As this which now I draw. . . . I see thee still,
> And on thy blade and dudgeon gouts of blood
> Which was not so before!"

And then, as if the blood upon the dagger had, by its horrid suggestiveness, steadied his brain, reason once more resumes her seat and denies the apparition, by asserting—

> ". . . There's no such thing ;
> It is the bloody business which informs
> Thus to mine eyes."

These false perceptions, these illusions and hallucinations, while they do not necessarily indicate any mental unsoundness, have been, however, the fruitful source of those apparitions, whether of demons, fairies, or ghosts, which have added to the credulity of man, intensified his superstitions, and made possible the organization of human error under such forms of belief as are typically illustrated by witchcraft and spiritualism.

So long as an individual is conscious that the illusions and hallucinations of his senses are unreal—merely "such stuff as dreams are made of"—the intellect is not affected; but when the false perceptions are accepted as realities, the mind itself is then involved, and a delusion or a false belief is said to exist.

A delusion may be based upon false perceptions; faulty ideas from perverted reasoning about real events, or from mental inability to distinguish differences in things.

A false belief is not, however, of itself indicative of insanity, so long as it is in harmony with the individual's common mode of thought and with the spirit of the age. This is apparent when it is remembered that witchcraft —now regarded as a delusion—was, not long since, held to be a truth; indeed, such master-minds as Bacon, Jewel, Luther, Calvin, Wesley, Blackstone, Coke, and Dr. Johnson, in accepting as a truth that which we now know was a mental epidemic of error, reflected only the universal belief of the age, and were free from any taint of insanity.

That the standard of mental health is variable because it is conditioned by race, age, environment, and circumstances, is abundantly attested by the history of the past; and this fact should be recalled in discussing the kinship of genius and madness.

The popular literature relating to genius and insanity is so meager and fragmentary that the recent contribu-

tions by Mr. Sully, on *Insanity and Genius* and *Genius and Precocity*, and by Miss Sanborn on the *Vanity and Insanity of Genius*, are as welcome as they are interesting. It is obvious, however, that names are often used to show the kinship between insanity and genius which do not represent the most illustrious minds. Mr. Sully is, however, logically correct in thus using names, for he includes under the term genius "all varieties of originative power, whether in art, science, or in practical affairs"; but in so doing he destroys, as it seems to me, the value of his argument in support of the relationship of insanity and genius, for, measured by this standard, the evidence is overwhelmingly against the theory. Neither is due regard given to the real significance of false perceptions, which are often made to appear indicative of insanity, when in reality mental integrity is not impaired.

Although obliged to follow a common trend of thought with familiar illustrations, it is, nevertheless, my hope to place a few garlands of honor on the brow of health, and to defend genius against the implication that it is such only with madness. The profound ignorance of the ancient philosophers concerning the nature of mind itself justifies us in attaching but little importance to their interpretation of its phenomena.

Thus, Plato's *Psychology* affirmed a self-existent, self-moving, and eternal soul, in form "like a pair of winged steeds. . . . In divine souls both steeds are good, in human souls one is bad. . . . Before entering the body the wings are lost which were nourished by beauty, wisdom, goodness, and all that is divine. . . . The mind of the philosopher alone has wings; he is ever initiated into perfect mysteries, and his soul alone becomes complete. But the vulgar deem him mad and rebuke him; they do not see that he is inspired. This

divine madness is kindled through the renewed vision of beauty. . . . Love itself is madness."

The soothsayers, or diviners, to whom Plato ascribed the "nobler madness," were regarded mad, not only because of their wisdom, but because of their extravagant rage and noisy behavior.

Virgil describes the inspired priestess as full of enthusiastic rage, and fiercely raving in her struggle to disburden her soul of the influence of the mighty god. Indeed, raging, foaming, and yelling, accompanied with antic motions, was the usual way of expressing the influence of inspiration or "possession."

Since Aristotle held psychological views similar to those of Plato, his saying that "it is the essence of a great poet to be mad" adds nothing to the strength of the theory.

The "madness," referred to in the conversation between Horace and Damasippus, did not specially relate to intellectual conditions, or to what we know as insanity, as has been intimated, but rather to individual and social ethics. The *Satire* says: "The school and sect of Chrysippus deem every man mad whom vicious folly or whomsoever the ignorance of any truth drives blindly on. This definition takes in whole nations; this even great kings; the wise man alone being excepted. . . . Whoever is afflicted with evil ambition or the love of money; whoever is smitten with luxury, or gloomy superstition, or any other disease of the mind, . . . come near me, in order, while I convince you that you are mad. . . . Whoever shall form images foreign from truth, and be confused in the tumult of impiety, will always be reckoned disturbed in mind; . . . where there is foolish depravity, there will be the height of madness. He who is wicked will be frantic too."

I confess that, with such statements before us, it hardly seems necessary to discuss the value of ancient opinions

on a subject which must be treated under the restrictions of modern definitions. We will, therefore, examine the question from the standpoint of more modern times, when the supernatural agency in insanity gives place to the deteriorating influences which unite it to other forms of nervous disease; and genius becomes a product of an age, in the expansive growth of the human mind.

That these extreme forms of mental expression are often associated, there is no doubt; and that genius is, at times, shadowed by mental disease is a fact well known; but our interest centers in the inquiry, whether this relationship is such an essential one as to justify Dryden in asserting—

> "Great wits are sure to madness near allied,
> And thin partitions do their bounds divide."

Or Shakespeare, when he says:

> "The lunatic, the lover, and the poet
> Are of imagination all compact."

In support of this essential union, Montaigne, Diderot, Pascal, Lamartine, and others, have subscribed their names, but in terms more general than specific, and with more rhetorical beauty than philosophic strength; while Moreau boldly affirms that genius is a nervous disease.

Charles Lamb, himself at times oppressed with mental gloom, stands almost alone in defense of "the sanity of true genius." With this view I am in accord, and, that the justification of this position may be seen, I desire to review the facts commonly cited against it.

Sophocles—poet, statesman, commander—was obliged to make a defense against the charge of insanity, instituted by ungrateful and avaricious children. He answered by reciting the tragedy of *Œdipus at Colonos*, which he had just finished, and he then asked the judges if the author of such a work could be regarded as mad. The reply was, "No!" and he was acquitted.

Lucretius—"writer of the purest Latin, and author of *De Rerum Natura*, the most exalted poem of the age" —whose mind combined the "contemplative enthusiasm of a philosopher, the earnest purpose of a reformer and moral teacher, and the profound pathos and sense of beauty of a great poet," has been used to illustrate the kinship of genius and madness upon the unreliable evidence that he lost his reason from the effect of a "love-philter" (a very ridiculous absurdity) which had been given to him; and after writing several books, during his lucid intervals, he committed suicide.

Were this allegation true, it could only show the baneful effect of the drug upon his brain, which is quite apart from the influence of any psychic cause. The historic facts are too few and insufficient to justify any statement as to the life and personal character of this man, who exerted such an influence over others by his writings, and yet, like Homer, was content to let his personality "pass through life unnoticed." Cæsar, Catullus, and Cicero were his contemporaries, and yet we know of him only through a brief record given by Jerome four hundred years after the poet's death. Independent of the historic doubts as to his insanity, the theory which makes a drug its potent cause, should at least find reason for not uniting to it his genius.

That Socrates had his "demon," or guardian angel, may be true; but, if so, the hallucination corresponded with the accepted belief of the age, and therefore signifies nothing against his mental integrity.

Neither is there justification in using such illustrious names as Descartes, Newton, and Goethe, to prove that madness holds its court so near the temple of greatness.

It is true that Descartes, in an hour of deep intellectual abstraction, was "filled with enthusiasm, and discovered the foundations of a marvelous science"; and, it may be that, during his profound meditations, in which he

"turned the eye of reason inward upon itself, and tried to measure the value of his own beliefs," an idea became so dominant that the sense of hearing responded to its impression, as if a voice from without had called him "to pursue the truth." In no way, however, did this simple and momentary hallucination interrupt the great work of his life, and in no lawful way can it be interpreted as an expression of a morbid mind.

That Goethe once saw his own counterpart approach him, I doubt not; but that this false perception, this passing incongruity—a mere incident of poetic revery when the mind, self-absorbed, wandered in its fancy—should be classed as evidence of a pathological condition, and made to bear witness against the healthfulness of Goethe's mind, is an assumption extravagant and absurd.

That Newton was once "decidedly insane," as some allege, is doubtful; and that he ever suffered from any mental disturbance which justifies the inference that his genius was allied to madness, I hesitate not to deny. Mr. Sully says: "The story of Newton's madness, which is given by a French biographer, and which is ably refuted by Sir David Brewster, may owe much of its piquancy to what may be called the unconscious inventiveness of prejudice."

The facts, as I gather them, point to a congestion of the brain—which culminated in a brain-fever—the result of overwork under unfavorable hygienic conditions. Newton himself refers to his illness in a letter to Mr. Pepys, and again in a letter to Locke, wherein he mentions his loss of memory, and his sleepless nights. It is not strange that his illness should excite the fears of his friends, not only for his physical but for his future mental health. Mr. Pepys expressed his anxiety to Mr. Millington, who replied that he had recently seen Newton, who was then well, and that, although his illness had caused "some small degree of melancholy, there is

no reason to suspect it hath at all touched his understanding." Huygens, a contemporary scientist, says, in a letter to Leibnitz in 1694, that Newton was ill for about eighteen months with phrenitis or brain-fever, from which he recovered by the use of medicines. With these data before us, it is a misconception of physiological and pathological facts to assert that Newton was insane: and that there was a kinship between his mighty genius and madness, is contradicted by the intellectual work which has given immortality to his name.

The star seen by Napoleon, which was to him an omen of success; the vision which came to Cromwell, and spoke the words prophetic of his greatness; the apparition which uttered the ominous words to Brutus—"I am thy evil genius, thou wilt meet me at Phillipi!" the dreams and visions of Benvenuto Cellini; the "trees like men walking," as seen by Sir Joshua Reynolds, and the appearance of the devil to Luther—are all examples of hallucinations which are entirely consistent with reason, and are not justly indicative of insanity or mental disease. They represent a habit of mind which naturally, under conditions of concentrated attention, intensified in an age tolerant of all forms of superstitions, "seeks for and creates, if need be, with or without consciousness, an outward object as the cause of its feelings." Luther, for example, saw with his "mind's eye" the image of the devil, which, in that age of religious excitement and credulity, was ever expectant in all minds, and generally present everywhere. "Hallucinations were," says De Boismont, "in the whole social community, not in individuals"; and hence it was that under the dominion of a general belief, however vague or irrational it may have been, the individual mind "demanded of imagination the realization of the phantasms of its dreams; and imagination, despite of resistance of reason, endowed them with form and sub-

stance." Herein is found a distinguishing factor "between real insanity and the separate phenomena of genius and moral exaltation."

In further support of this opinion we may cite the hallucinations of Loyola, when he heard celestial voices; of Edward Irving, who received the gift of prophecy and the "power of tongues"; of Dr. Johnson, when the voice of his dead mother came to him; of Malebranche, when the deep feelings of his soul were to him the audible voice of Diety, and of Joan of Arc, who, under the guidance of saints, led the French arms to victory.

That genius "has its roots in a nervous organization of exceptional delicacy," is undoubtedly true, but it does not necessarily follow that the liability to mental discord and confusion is thereby increased, because this delicacy of brain-structure and its functions are admirably adjusted, and the very perfection of the mechanism enables it to work with the least possible friction or injury.

Under certain conditions, however, we have eccentricities of thought, feeling, and action, which indicate an unstable condition of nerve-element; but it does not follow that this instability necessarily impairs the integrity of the mind; much less does it imply that "genius," more than the lower expressions of mental power, is nearer the border-land of mental disease. I doubt not that permutations of this unstable condition may occur which, by supplementing the natural gifts of mind, cause a variety of individual traits, which may give to the poet Campbell indecision and indolence; make Carlyle cross and pessimistic; Byron proud, generous, and reckless; Schlegel foppish in his vanity; Keats despondent; Pope crafty and pretentious; Swift satirical, avaricious, and irascible; Chateaubriand egotistic and vain; Burns and Poe convivial and intemperate;

Eliot sensitive and dependent ; Hawthorne shy and modest ; Wordsworth simple-hearted yet full of conceit ; and Ampère absent-minded and unpractical. Thus might I show certain peculiarities which belong to the personal mental outfit of almost every one whose individuality is sufficiently marked to make him worthy of notice ; but these peculiarities or eccentricities are not essentially morbid, neither do they give affirmative evidence that genius is related to madness. Such peculiarities belong to all orders of mind—the humble as well as the exalted—and cannot, therefore, have an exclusive application.

Add to the personal eccentricities of Pope, Byron, Johnson, Carlyle and Swift, the temper which at times became in them extravagant rage, and the proof is yet no stronger that genius and insanity are but different types of mental disease ; for passion and appetite are, in all their forms, expressions of organic life, and common to humanity, and, therefore, as universal factors, they can not be dissociated and made to bear witness either for or against the subject before us. It has already been admitted that eccentricities of character imply a want of mental poise or equilibium, which is even more apparent in the extravagant passions which at times hold individuals under despotic control, and often indicate decided moral obliquity. This I do not deny ; but yet affirm that the violent passion at times observed in one of exalted powers of mind is no more evidence in favor of the kinship between these powers and mental disease, than is the same passion, when displayed in a low and vulgar mind, proof that stupidity is a congener of madness. Mr. Madden is quite as justified in asserting that " the maladies of genius have their main source in dyspepsia," or I in affirming that, because some eminent men have been physically puny and ill-formed, therefore their

genius is related to, and dependent upon, bodily imperfections.

In trying to establish the kinship between mental greatness and disease, Mr. Sully states, what I do not deny, that "a number of great men have died from disease of the nerve-centers," naming Pascal, Cuvier, Mendelssohn, Mozart and Heine—none of whom, however, were insane.

That genius should be subject to "all the ills that flesh is heir to" challenges neither surprise nor dissent; but to hold this as evidence in support of the idea that "the extreme mind is near to extreme madness" is, as it seems to me, an erroneous interpretation of physiological and pathological facts. To prove that Pascal died in convulsions from an acute brain trouble, in connection with a disease of bodily organs, and that Mendelssohn and Rousseau died of apoplexy, and Heine of spinal disease, is not proof that there was any essential weakness or disease of nerve-element, but rather is it evidence of disease of blood-vessels through faulty nutrition. When hemorrhage occurs in the brain, its substance is disorganized, as it might be if any other foreign substance were forced into it, and nervous disturbance very naturally follows; and possibly secondary nervous or mental disease, but it is not correct to speak of the primary apoplexy as a disease of the brain, or to infer that, because a person of high mental endowments has thus suffered, therefore, as Lamartine says, "Genius bears within itself a principle of destruction, of death, of madness." Cuvier, after a life of incessant intellectual toil, with mind unclouded, died at the age of sixty-three, from paralysis of the throat and lungs. Kepler, sound in mind, died when sixty years old of fever, which some say caused an abscess in the brain. The cause of Mozart's death is unknown; his sickness was of short duration. He thought himself poisoned, but the facts were hidden

in the pauper-grave wherein his body was unkindly thrown. Now, I protest that such cases give no evidence of insane temperament, and in no way illustrate the kinship between mental greatness and disease.

Again, it is said, and often with reason, that this kinship is shown by the suicidal impulse, which "is only another phase of insanity." That suicide or homicide may result, under this impulse, I doubt not; but to make this fact of special value its numerical proportions should at least be such as to make it a factor of constant value. Because Goethe, Chateaubriand, George Sand and Johnson have said that at times they felt an impulse to commit suicide; because Beethoven, Schumann, and Cowper, who were at times morbid, really made the attempt; and Kleist, Beneke, and Chatterton succeeded in self-destruction—we are not justified in saying that the impulse or the act itself came because genius contains an element of madness. Hundreds who commit suicide every year do not possess genius; why, then, make it the responsible agent for the few?

It is charitable to think that the misdeeds of our friends, or of those whom we admire, are covered by the plea of irresponsibility through insanity. Science, however, deals with facts, not sentiments. That mental and motor impulses often occur which, because they are stronger than volition, regard not the consciousness of right or wrong, there is no doubt; that these impulses are very frequently the product of a morbid mind is also well attested, but the question before us is limited to the relationship which genius bears to suicide, as one expression of madness.

I confess I can see only that relationship which exists in the organic necessities which constitute the foundation of human thought and action, and not the psychological relationship which makes the exaltation of mind the destroyer of its life.

The regularity and constancy of results which spring from the varying conditions of race, climate, and occupations, as well as from the social, political, and moral influences around us, clearly indicate, what statistics prove, that madness is but one of many causes of suicide. Now, since genius is itself exceedingly rare, and its union with insanity still less frequently found, it is evident that suicide, although occasionally committed by those of exalted minds, is altogether too infrequent among them to justify us in claiming it as evidence in behalf of the insanity of genius. Certainly, when we find that Kleist, Beneke, and Chatterton stand almost alone in this list, the support for the assumption is not strong, nor is it enhanced if the quality of genius thus represented is duly estimated.

Those who would make genius dependent upon or associated with a morbid mental state, seek to strengthen their position by citing the names of Burke, Chatham, Linnæus, Moore, Southey, Scott, Swift, and Shelley, as among those whose faculties were impaired by mental disease. I interpret the facts differently.

It is true that Linnæus at the age of sixty failed in memory, and that, when nearly seventy, an attack of apoplexy ruined his mind; that Moore, Southey, and Scott, when the years of their life were nearly numbered, were enfeebled in mind, because in old age the work of repair had failed; while Swift, at three-score years and ten, lost his mental powers as a result of a disease which began long years before, not in the brain, but in the organ of hearing.

Shelley, indeed, was eccentric and given to sleep-walking and hallucinations, and at times he may have confounded the mythical with the real, seeing "forms more real than living man," but I know of no rule of psychology or of medical jurisprudence which will authorize us to say he was insane. His fervor, his reason,

and his imagination conspired to lift him into the higher realms of an idealism which was the antithesis of things as commonly seen, and his mind grew and strengthened until, at the age of thirty years, obedient to the "infinite malice of destiny," he died.

Because Lord Chatham suffered at one time from melancholy, the direct result of suppressed gout, it in no way proves that his genius was allied with madness, for the same clinical facts are observed in all orders of mind.

Since, therefore, every degree of mental disorder from the simplest feeling of depression to the wildest mania, regardless of the quality of mind, may follow from the undue retention in the blood of the waste products of tissue-change, either alone or combined with other morbid bodily conditions, there seems to be but little justification in asserting that "there is no great genius without a mixture of insanity."

In the history of political literature the name of Edmund Burke stands among the first, and is representative not only of that illuminating power which belongs to lofty minds, but of the genius which comes "as the consummation of the faculty of taking pains." Year after year his voice sounded in behalf of the "sacredness of law, the freedom of nations, the justice of rulers," and the imagery of his thought—imposing in its majesty —"carries us into regions of enduring wisdom." For nearly threescore years his mind retained the dignity and calm of lofty greatness, and seemed to totter from its balance only when he breathed the torrid heat of fury which was sweeping over France and gathering wrath against the horrid atrocities of "'89." He had before him the vision of Marie Antoinette, "glittering like the morning star, full of life and splendor and joy," and felt that, through unbelief and passion, the props of stable government and morals were being broken and de-

stroyed. The "divine right of kings" was yet an article of common faith, and he saw their sorrow, but heard not the wail of anguish which ascended from the oppressed and starving people. Rage against the lawless Parisian mob filled him, and in his wrath he spoke as if envenomed hate had made him mad; and he was so adjudged, but only by those who differed from him. The inspiration of his genius gave him the tongue of truth, and the penalty was an assault upon his sanity.

Then came the supreme sorrow of his life, the death of his son, and in his grief he wrote: "The storm has gone over me, and I lie like one of those old oaks which the late hurricane has scattered about me; I am stripped of my honors; I am torn up by the roots, and lie prostrate on the earth. I am alone, I have none to meet my enemies in the gate; . . . I live in an inverted order. They who ought to have succeeded me have gone before me; they who should have been to me as posterity are in the place of ancestors."

Because of the outward expressions of grief which were at times his, as when his son's favorite horse came to him and put its head upon his bosom, which caused Burke to cry aloud in his sorrow—because of such manifestations of grief, it is said Burke was mad. Edward Everett has well said: "If I were called upon to designate the event or the period in Burke's life that would best sustain a charge of insanity, it would not be when, in a gush of the holiest and purest feeling that ever stirred the human heart, he wept aloud on the neck of his dead son's favorite horse." As proof that his intellect was not disordered, his *Letters on a Regicide Peace*, written in 1796, a year before his death, bear ample evidence, and are regarded, says John Morley, "in some respects the most splendid of all his compositions. . . . We hardly know where else to look either in Burke's own writings or elsewhere for such an

exhibition of the rhetorical resources of our language."

That Burke had at times eccentricities and fleeting aberrations may be true, but to call such a man insane, or to speak of him as illustrative of the kinship between genius and madness, is to make sport of facts and mockery of human thought.

Notwithstanding the many names I thus take from the roll-call of madness, there are, nevertheless, many gifted minds that have not been absolved from this sad heritage, or been able to bear with calm serenity the misfortunes and burdens of a weary life. Such was Schumann, the eminent composer of music; and Blake, to whom the realities of the world were but dissolving forms of his own consciousness; and Clare, who, when his melancholy was deepened by the neglect of family and friends, wrote so plaintively of his own gloom and loneliness. Cowper was timid and morbid, and agonized under religious melancholy and suicidal impulse. The insane temperament was also definitely marked in Comte, the oracle of the "Positive Philosophy"; in Tasso, whose melancholy fate gave to Goethe the opportunity to picture a psychological drama, wherein character is revealed under the glow of "poetic furor," and also, at times, oppressed by morbid fears and delusive visions; in Swedenborg, whose prolific mind teemed with fancies and speculations, contradictions and absurdities, which can only be explained on the theory of a mind diseased; and in Charles Lamb, whose "diluted insanity" cast an enduring shadow over his life.

The facts, as I view them, make me dissent from the theory that a diseased brain is the physical substratum of genius, or that the possession of such exalted mental endowments "carries with it special liabilities to the action of the strong disintegrating forces which environ us." "A large genius," says Dr. Maudsley, "is plainly not in the least akin to madness; but between these

widely separated conditions a series of connections is made by persons who stand out from the throng of men by the possession of special talents in particular lines of development; and it is they who, displaying a mixture of madness and genius at the same time, have given rise to the opinion that great wit is allied to madness."

To the extent that a nervous organization makes possible excessive emotional life, or vagaries in thought or action, to this same extent is true genius qualified and limited; for without calm reason and volitional control creative imagination is distorted into an irresponsible fancy.

The degree of perfection of any mechanism, whether it be a watch, an engine, a harp, a telescope, or the human brain, is the measure of the quality of the work which can be produced therefrom; and, conversely, the quality of the work is an index of the structural character of the instrument employed.

The better the finish and adjustment of a mechanism in its various parts, the less will be the friction, and the "wear and tear" from constant use; and, although the very delicacy of its adjustment may give a greater susceptibility to disturbing causes, the causes themselves are not inherent but incidental. These facts apply with equal force to that most perfect and complex of all known mechanisms—the human brain, which is energized with the subtile principle of life, and evolves thought, feeling, and will, which, in their noblest and most exalted expressions, are indicative not of disease but of mental health.

Nervous and mental diseases are too common among all classes of people and orders of intelligence, to permit us to think that genius is the special object of their dominion. This idea is rejected, not because it is repugnant, but because it is not sustained by facts when measured by the standard of the highest art, or loftiest thought, or greatest work.

Look at the scores of eminent names within a hundred years, and show therefrom, if possible, the evidence which justifies the statement that intellectual greatness, is "beset with mental and moral infirmity," or that genius is merely an expression of a morbid mind, akin to madness.

Imagination gives to genius—which is the intellectual scout of progress, and the Titan force which organizes the factors of civilization—a realm wherein the soul throbs and burns with the fervor which comes only when a new truth cleaves the darkness and illumines a pathway hitherto unrevealed ; and where the clash and turmoil of cerebral action excites the highest pleasure, though at the same time they often weary and exhaust.

In this century, when the fierce blaze of modern thought has filled the world with unparalleled glory, and the inventive genius of man has made the earth a vast workshop of industrial arts, wherein the human brain is "master-workman" over all, how rare is it that the brain-worker feels the oppression of a "mind diseased," except when, like the wage-worker, he frets and worries under the burdens of a weary life, and falls by the wayside because the struggle for existence—keen, sharp, and relentless—has taken from him the inspiration, the strength of hope!

Mental stagnation, personal or domestic sorrow, social inthrallment, religious excitement, crushed hopes, and poverty, are the chief moral causes which contribute so largely to the mental infirmities of man.

In conclusion, I hesitate not to say that the most illustrious names of ancient or modern times—in all departments of human thought or activity—have been, with but few exceptions, loyal to the sovereign rule of sane reason ; and the sweep of their imagination has been in curves which rounded in the bright empyrean of truth and beauty.

JANUARY 4, 1887—THIRTY-FIFTH REGULAR MEETING.

William G. Stevenson, M.D., president in the chair.

Twenty members and one hundred seventy-five guests present.

James M. Taylor, D.D., and Mr. James Winne were elected active members.

James M. De Garmo, Ph. D., gave an address entitled "Pre-Historic Man in America," by which it was sought to establish by varied forms of evidence the fact of man's existence on this continent long before there were any historic accounts.

The usual term, "paleolithic" man, was avoided, and the issue kept clear on the pre-historic basis. The existence of skeletons or parts of skeletons under strata not made in historic times was taken as substantial proof. Then the refuse heaps along the Atlantic coast, of vast piles of shells, gathered long before the Indians lived here, was still further adduced as evidence. And then the mounds and fortifications of the mound builders, with their pipes and pottery, their copper ornaments and discs representing the sun and moon, the proofs of a civilization in copper mining and copper working, with something of a knowledge of astronomy from the telescope tube of obsidian, were all considered as incontestable witnesses of a mighty population inhabiting this country, in times long before any authentic records would indicate.

The singular absence of domesticated animals was noted, as was the building of the mounds before the last fluvial subsidence—the last river terrace never being occupied. The similarity of the work of the mound builders, with some Mexican remains and some in Central America and Peru, would seem to indicate a community of origin, or a close race affinity. All these facts were treated as the real remains of a people or of many peoples, who have left them to tell the story of their existence to other generations.

FEBRUARY 1, 1887—THIRTY-SIXTH REGULAR MEETING.

William G. Stevenson, M.D., president in the chair; two hundred members and guests present.

Rev. Edward Riggs, of Marsovan, Turkey, gave the following address entitled:

"RUINED CASTLES IN ASIA MINOR."

Mr. President, Ladies and Gentlemen: Our subject this evening pertains to the fascinating region of the Orient, but where is the subtle line which divides "the east" from "the west"? The abstract scientist will readily answer that there can be no such line. But in defiance of this verdict the tourist has discovered the mystic boundary, and he reports that the line passes in a somewhat irregular northeast and southwest direction just to the west of Constantinople. When the traveler leaves the Danube at Rustchuk and finds himself on a real Turkish railroad, creeping slowly towards Varna on the Black Sea, a constantly increasing multitude of queer, contradictory, characteristic points bring cumulative evidence of the fact that he is crossing that mysterious line and entering the dreamy realm of the Orient. At the Black Sea port he finds himself in a babel of languages, and for some inexplicable reason, in getting on board the Lloyd steamer, he experiences more confusion, delay and petty annoyance than he has encountered in his entire journey across Europe. A quick run of a dozen hours brings him to the entrance of the Bosphorus. As he enters the narrow strait which divides two continents, a beautiful landscape meets his eye. The heavy swell of the Black Sea changes abruptly to the perfectly steady quiet of harbor water, while the dark blue waves still dance with a short and merry plash, under the influence of the deep, swift currents, that sweep on down through the channel. The eastern and western shores, constantly approaching nearer and

nearer to each other present a pleasing variety of green hillside, luxuriant grove and garden, and rocky shore or sandy beach But high above all other objects, perched on the summit of the abrupt hill on the Asiatic side, a venerable ivy-clad castle secures the attention of the beholder. Its massive walls are crumbling and gray with age, its ports and gateways long since innocent of bolts or panels, yet still its grim battlements look down with a suited but benignant scowl upon the white-winged craft that pass and repass beneath it. At the foot of the hill on which this castle stands, a rude modern building represents the authoritative outpost of the "department of public health." As your steamer approaches this spot the throb of the engine ceases and a small boat darts out from under the vessel's side and pulls away with a steady stroke toward the health office, bearing the health papers of the vessel. If you are unfortunate enough to fall upon quarantine times, here your steamer will drop anchor, and here you pass your dreary days of imprisonment. While the traveler lingers here,—be it an hour or a week,—he ever has before him the hoary outline of this grand old castle. He can admire it, and study it, and sketch it; he can stretch himself on the deck under the awning and watch the light clouds as they chase each other behind its tenantless turrets. In whatever direction he may turn to enjoy the varied beauties of the scene, his eye will always revert to this curious structure as the one prominent feature in the charming picture. But soon the measured pulsations of the steamer announce that you are again in motion, and you glide swiftly down with the current, turning back for one last look at that ancient ruin, and wondering whether it is the work of Turk or Greek, Genoese or Venetian. Quickly the green hills close behind you, and with every turn in this wonderful Bosphorus you find yourself apparently in a new and wholly land-

locked lake. The shores begin to be dotted with residences which cluster together at certain points in quaint-looking villages, nestling down amid a rich luxuriance of trees and vines. Most striking among the fresh green foliage are the brilliant rose-colored masses of the curious judas tree, while the graceful stone pines rise majestically on the crest of the hills. Neat modern kiosks peep out from amid the garden shrubbery, and the palaces of ambassador and pasha occupy choice positions on the very edge of the water, while an occasional minaret raises its slender peak, bringing to mind that strange faith which prevails in this seeming fairy-land. The "Giant's mountain" rises high on your left, where Mohammedan tradition seats the Bible hero Joshua, and depicts him, from this lofty seat, bathing one foot in the Black Sea and the other in the Bosphorus. In an enclosure on the top of the mountain a significant-looking mound, measuring some thirty-six feet in length, is called his tomb, but the assiduous attendant explains that it contains only his head. Nearly opposite this point, on your right, stretches away a wide plain, in which Peter the Hermit, is said to have encamped his motley horde, eight hundred years ago, and in the middle of it stands an immense plane-tree, which tradition identifies as the very one under which that leader's tent was pitched.

As your steamer sweeps gracefully around another point and opens up another panorama, there rises directly in front of you another and a grander pile of ancient castle. Enormous stone towers, some round, some square, and some hexagonal, follow each other up the steep hillside, connected together by huge parapeted walls. Each of these immense towers has a double row of battlements, the inner parapet rising above the outer, and their heavy walls are pierced with slender loop-holes. This chain of walls and towers enclose a large irregu-

larly shaped space on the very point of an abrupt promontory and reaching from the very water's edge clear up to the brow of the hill. This enclosure is now filled with dwellings, the battlements are dismantled, and the heavy portals unhinged, only a small part of the enclosure being used for modern military purposes. This remarkable structure bears the name of the "Castle of Europe," and directly opposite it stands the "Castle of Asia," which enjoys a less striking location, but is still a formidable looking fortress. Many are the traditions about these strange old ruins. Their origin is ascribed in turn to each one of the successive powers which have controlled this important strait. But whether built by Genoese or Turk, as military defences they belong to an age long past, and are now chiefly interesting from their extremely picturesque appearance, their strange grouping and still perfect symmetry forming a very striking and attractive outline against the rich colors of that oriental background, and their gray walls, presenting a most pleasing contrast to the dark cypresses which stand guard over the dust of the "faithful" who lie buried at their feet.

If we take this splendid ruin as the type of this old and crumbling monarchy with its darkness and traditions of the past, we have not far to seek also for the symbol of the bright future of an enlightened and reformed people. Just back of the old castle, on still higher ground, and commanding a superb view of one of the most magnificent panoramas in the world, stands that monument of American enterprise and philanthropy, —founded by a citizen of New York—Robert college. With its fresh walls and clean-cut angles, its mansard roof and cheerful windows, it is indeed a most picturesque contrast to its venerable but useless neighbor. The ancient and mediæval is mouldering rapidly away, while all that tells of hope and cheer in the future for

that dark and unfortunate land clusters about the recently established progressive Christian institutions of education and religion.

But your steamer hastens on, and you turn from this remarkable group of buildings, old and new, to scan the ever thickening beauties all about you. For another half hour you sail along down between rows of glittering palaces along the water's edge with a widening belt of densely packed suburban quarters covering the steep hills back of them, until at length your vessel swings into her berth under the very walls of the city of the Constantines. Here you have, not a detached castle or group of towers, but a great city entirely encircled by a continuous chain of huge towers connected by massive walls. About two-thirds of this circuit have the additional protection of the sea or the harbor. On the land side, to compensate for this, there was a triple wall. Much of this still stands in ivy-clad majesty, though huge fissures yawn in many places, the dread work of repeated earthquakes, and long stretches of what was once a deep fosse between the outer and the middle wall is now a luxuriant vegetable garden. There are six gates on this landward side; each one has its own special features of interest and each is flanked by great towers, while above them all cluster innumerable reminiscences and traditions of blood and violence, of seige and assault, of heroism and of villany.

At the southwest angle, where the land wall runs down to the Marmora Sea, is a most interesting group called the "Seven towers." Here the ancient builders put up tower after tower with lavish prodigality, as if, forsooth, by their very numbers these structures could support each other and render absolutely impregnable this critical point in the defences of the city.

These are some of the castles in and near the great capital, and with such a superficial glance many trav-

elers are satisfied. If they would only turn aside a little out of the well-worn path of the standard guide books, they would find Asia Minor filled to overflowing with points of deep interest. Centres of ancient civilization, scenes of memorable battles, homes of renowned men, and relics of wonderful architecture and sculpture lie waiting to be recognized by the modern world when it shall get wearied of its present favorite haunts. Ancient history, classical history, mediæval and modern history, secular and ecclesiastical history, each has left its distinct marks upon this much reconquered land. Thoroughly interwoven with the past life of Asia Minor are the names of Homer and his heroes, of Cyrus and Crœsus, of Darius and Alexander, of Strabo and Diogenes, of Mithridates and Julius Cæsar, of St. Paul and St. John, of Timothy and Polycarp, of Constantine and Julian, of Gregory and Basil, of Genghis Khan and Tamerlane, of Othman and Bayszid, and a multitude of others.

But were all historic interest set aside the country would still be worthy of careful attention and study, for aside from the deep interest attaching to its present inhabitants there are broad fields for investigation and enjoyment in its natural scenery, its geology, its fauna and flora and its climatic characteristics.

There is, however, scarcely anything which will strike the ordinary traveler so remarkably as the numerous castles which meet the eye. Modern warfare has developed the use of gunpowder to such a degree that the old-fashioned castle holds a much less important position in relation to other means of offence and defence than it did in ancient times. Yet, that it has not wholly lost its power and usefulness, witness such important castles as are familiar to all, or that perhaps less known but not less heroic castle at Kars on the border between Russia and Asia Minor. This remarkable fortress, after

a long siege by the Russians in 1855, during the Crimean war, was at last reduced only by starvation, and in 1877 was carried by assault by the same enemy, after many efforts and at a terrible cost of life. By the settlement which followed the latter war in 1878, this much-coveted prize was allowed to remain in the hands of Russia.

But the castles which, like this one, are still held as highly important by modern military art, are the exceptions, and are rendered worthy of such distinction by some peculiar advantage of position or construction. The great mass of those elaborate structures on which past ages have expended such untold energies, stand now, splendid relics of a bygone age, but substantially useless for modern practical purposes.

Asia Minor is full of such wholly or partially abandoned castles. They were the pride and dependence of former times, and much skill and taste were displayed in the choice of sites for them. Perched on abrupt and lofty eminences they look down with patronizing dignity at the clustered dwellings at their feet, and have in many cases decided the location and even the character of a town. The geological structure of the country favored this passion for castle-building—mountainous and rugged, and full of abrupt cliffs and deep gorges, it furnished matchless sites ready to the hand of feudal lord or military ruler. In many instances the convergence of two mountain streams has determined the location of a settlement. These streams in long ages have cut themselves deep channels leaving between them a narrow and precipitous rock. This being seized upon by the military instinct of some local authority, a castle is the result, and this in turn causes the growth of the village into a city. In other cases in the very midst of some broad alluvial plain a huge rock will stand, like an island in the sea, as abrupt as Dumbarton castle or Gibraltar, and nearly as high. On this some local chief

has fixed his stronghold, and gradually about the base of it a town grows up.

These castles by their very location are rendered picturesque in the extreme. And this appearance is enhanced by many circumstances. Their long disuse has allowed them to fall into decay. Crumbling arches, and huge rents in massive masonry; broken columns and ivy-covered battlements produce endless variety of pleasing contrast. And then a new source of variety is in the different styles of structure employed in producing these results. These castles are not all the work of one dynasty or of one people. There is every stage of advance in the art of architecture, from the primitive and massive cyclopean efforts of prehistoric races down through the chaste and painstaking products of Greek skill and the mathematical nicety and utilitarian shrewdness of the Roman, to the lavish ornamentation of the Seljookian, and later still the servile imitation or vandal destructiveness of the Ottoman Turk. These different types sometimes stand alone as characteristic specimens, but more frequently, in the castles at least, they are piled upon one another in strange confusion, demanding careful examination to discover the line of division, but rewarding such patient investigation by furnishing indubitable testimony to the successive steps in the historic narrative.

A further point of variety is as regards their location with reference to the sea, some being on the coast, and others inland. Of the latter few ever fall under the eye of the tourist, and even of those on the coast, aside from those mentioned in the vicinity of the capital, the extensive castle on the hill behind Smyrna is about the only one known to the general traveler. But some of the most historic and picturesque are found on the Black Sea coast of Asia Minor.

The town of Trebizond, near the eastern end of this

coast, possesses a venerable castle, replete with thrilling historical memories. For two and a half centuries this was the seat of an independent Greek empire, the last struggling remnant of the great old Greco-Roman empire. And now, after four long centuries of Turkish domination, the inhabitants have a decidedly Hellenic cast of character, and the family language among the Greek portion of the community is still essentially the language of Xenophon, preserving, indeed, many archaisms which have been lost out of the more polished language of Athens and Constantinople. As you walk about the neglected ruins of that once famous fortress the native Greek who accompanies you will sigh over the miserable misrule of the present government, and glancing furtively about to assure himself that no Turk will overhear him, he will in a hoarse whisper, pour forth a rapid torrent of invectives against this unrighteous usurpation, and tell you of the secret hopes which his people cherish of an extension of the present Greek kingdom till it achieves a restoration of the old boundaries and prestige of the Byzantine empire.

Another noteworthy castle of the Byzantine period stands on a remarkable promontory at Sinope, which is located at the northernmost and central point of this Black Sea coast. This point of land runs out into the sea in such a position as to form the finest harbor on this entire coast. Unlike most such castles, this is not dismantled and deserted. The sentry paces constantly before the heavy, iron-bound gate, and the blood-red moslem standard floats over the patched and renovated walls and towers, while old-fashioned bronze cannon point their smooth bores through the deep embrasures. One reason for this vigilance is that this fortress is utilized by the Turkish government as a state prison. Were our theme at this time the fertile and somewhat startling one of Turkish prisons it would be in place

to look a little into the condition of the motley group of wretches, whose begrimed and pallid faces tell each a several tale of woe, and each a varying degree of depravity. But this is too large a subject to allow of even a glance in this connection. As the visitor stands on the ramparts of this restored castle he looks out on the dark and remorseless waves of the treacherous Black Sea. Yet, if he will turn his eyes to the other side of the rock on which he stands he will see spread out beneath him, a spacious harbor, placid as a mountain lake, and dotted with craft of every sort. In this peaceful sheet of water took place, within the memory of most of us, one of the most fearful tragedies in all the records of naval history. Russian habitual jealousy of the Turkish empire found a pretext in 1853 in certain differences of view regarding the relations of Christian sects in the care of the sacred places in Jerusalem, and the Crimean war was the consequence. The Turks got their fleet in readiness and started out into the Black Sea to take the Russians in hand. By the time they had been at sea a couple of days, so completely was the entire body of men prostrated with seasickness that they were obliged to put into port; and they hastily sought the safe and quiet harbor of Sinope, hoping to get their heads level and take a new start. Here the great double-headed Russian eagle swooped down upon them. The Russian fleet pursued them into the harbor and a fierce engagement followed. The Turkish fleet was absolutely annihilated with the exception of one ship, which fled to Constantinople to carry the woful tidings. I was, myself, in Constantinople at the time, and I well remember the thrill of horror and indignation which swept through the community upon receipt of the intelligence. The action of the Russians was held to be one of treachery, and it led ultimately to the western European powers entering the

arena as allies of the Turks. But the name of Sinope will always be associated with this sudden and terrible disaster to the Turkish fleet.

Half way from Sinope to Trebizond lies the small fishing town of Unieh. It is well known in ancient history as Œnóe, and is not far from the celebrated Thermodon where the Amazons exhibited their charms and their valor. While on the other side of it lies the region filled with iron ore, which was worked by the Chalybes of old. This ore, after yielding an unwilling tribute to those old workmen, was cast aside as useless, but in these days it is being remelted and forced, by more modern but still very rude processes, to give forth still further treasure. The town of Unieh, though built partly on a bold promontory, boasts nothing within its limits in the way of a castle, but the traveler will be well rewarded for a pilgrimage of about three miles to the castle of the region. Just east of the town a small stream empties into the Black Sea. The valley which brings this stream down from the high mountain range runs back a considerable distance inland, and is beautifully diversified with cultivated field and shady grove. Right in the midst of it, about a league from the shore, stands a wonderful perpendicular rock rising five hundred feet of sheer precipice from the level of the valley. On and about the summit of this cliff is perched an old castle, the site of which might make an eagle dizzy, and the construction of which must have been a herculean undertaking. The rear or landward side of this rock is connected with the mountain chain by a sort of neck on which a little village has tucked itself under the shelter of the fortress. But after you scramble up to this village you have a nearly perpendicular wall of over two hundred feet still above you, and the narrow foot path to the castle goes meandering about the face of the steep rock in a way

which tends to reconcile one to not being able to reach the actual summit. About half way up the smooth face of this upper part of the rock is seen a tetrastyle temple cut into the solid rock. This is now wholly inaccessible, the path to it having been entirely obliterated by time, and the villagers have no tradition of any one's having actually reached the highest point. Yet there, on the very top, stand the ruins of the innermost citadel of what was once undoubtedly a very strong castle. Its casemates, and walls, and bastions extend for some distance down the rock, and a heavy gateway a short distance above the village marks the lower limit of its fortifications. But who built it? What its date or purpose or history? Who can tell? There it stands; an embodied and unsolved enigma, giving the imagination free scope to account for its mysteries.

This castle contains one striking feature which may be alluded to here as characteristic also, of a number of other castles which we have yet to mention. It is a curious subterranean excavation, the purpose of which has been a subject of discussion, some supposing that these excavations were intended for secret entrances and exits, or for means of communication with outside parties in case of siege, while others give other and more improbable explanations. If a humble opinion may be allowed, it may be suggested that Strabo not only explains their object, but in his mention of those he was familiar with, he throws a flood of light upon the date, for example, of this castle, where the same structure is found as that which he describes, carrying us back at least two thousand years to find the original designer of these castles and their underground works. He calls them *νσρετα*-hydreia, that is, aqueduct or reservoir. Each of these—and they are found in all the castles of the kind which I have visited—consists of a large, deep, straight cutting, not perpendicular, but at about an angle of forty-five

degrees. It is about ten feet in diameter and goes down to a great depth. The bottom is always filled with stones and rubbish so that the exact depth cannot be measured in any instance, but some of them are still open to a depth of three hundred feet, and how much farther they went could only be ascertained by removing the debris at the bottom, thus emulating the almost incalculable labor of the original diggers. In some a spring of delicious cool water is found at the bottom, and in others the water has filled up the well to its very mouth, while others still are perfectly dry, as now partially filled up. The conclusion is that the original castle builders considered it a prime requisite to have an unfailing supply of water, and so they chose their spot and pushed their shaft down and down till they struck water, which, once reached, would never fail them. The angle was undoubtedly chosen to facilitate descent, and for this purpose steps were originally cut, all the way down, though these are for the most part obliterated or covered up. An angle of forty-five degrees, however, without steps is not an easy grade to ascend or descend, over loose stones and sand and earth moistened by the underground dampness. Hence the ludicrous is not wholly lacking from the experiences of such curious persons as insist on exploring these caverns. As when, with lighted taper, after a few cautious steps, you find yourself sliding rapidly and irresistibly downward into the darkness in some preposterous position, or reaching out to grasp what appears like a projection from the rocky wall, you find you have clutched and carried away a bat, who was innocently sleeping, suspended, head downward, by his claws. The rock on which this castle is built consists in part of a coarse conglomerate, containing large pebbles of all varities of agate, carnelian, jasper, onyx and chalcedony. And it is an interesting coincidence that

Mithridates the Great, who ruled in these parts a hundred years before the beginning of our era, is said by contemporaneous writers to have possessed a rare collection of gems in this line,—cups and ornaments of jasper and amethyst, etc. These were doubtless produced near this spot, and I have picked up both there and along the neighboring sea coast, pebbles of these materials of striking beauty.

One more of these castles on the Black Sea coast deserves special mention. Farther to the east than Unieh and not far from Trebizond is the town of Kerasus, from which name, by the way, we derive our word cherry. This place is now the chief centre of the filbert-growing industry, the whole country round about being devoted to the raising of this toothsome nut, of which thousands of sacks are shipped annually to southern Russia and elsewhere. A huge mass of igneous rock juts out into the sea, forming a small, open harbor on each side, and the town is situated on the neck of this precipitous promontory. On its summit are the ruins of another of these old castles, and its claims to very great antiquity are well authenticated. This castle was first built by Pharnaces, grandfather of the great Mithridates, and strengthened and improved by the latter. Here he placed his sisters and his wives during his wars with Lucullus the Roman general, and when hard pressed by the Romans he sent his steward Bacchides to the castle at Kerasus, with the request that he would kindly strangle those ladies rather than let them fall into the hands of Lucullus. This delicate mission he performed, but whether in such a way as to please the ladies or not the historian fails to state.

In one corner of this castle enclosure the local Turkish court holds its sessions, and the learned Kadi placidly strokes his long beard, innocently ignorant of the potent precedents so close at hand for the injustice which he dispenses.

When you leave the sea-coast and strike inland you soon find that you have not left the castles all behind you. On the great post road from the seaport of Samsoon to Bagdad, the first large town, sixty-five miles from the sea, is Amasia, the chief capital of Mithridates, and birthplace of that most enterprizing of ancient geographers, Strabo. His description of the castle and its surroundings would answer for a page of a modern guide-book, so accurately and in detail does he set forth many points just as they are. The river Iris at this point winds its way tortuously through a deep gorge between high, steep cliffs, like the cañons of some of our western rivers. Down in the very bottom of this gorge lies the city of Amasia, the victim of furious winds in winter and fierce heat reflected from the rocks in summer. Near to the narrowest point in the ravine the river is joined by a small tributary, which at a sharp angle cuts its way through the hills, leaving between the two rivers a narrow rock six hundred fifty feet high, with two faces almost perpendicular, and only accessible on the rear where it joins the mountain range. Here the Kings of Pontus built their castle, and here they defended their treasure and asserted their authority. And truly a noble site it is! commanding a view not only of the river and the city immediately beneath, but far up and down the several valleys which converge here, and are filled with an almost tropical wealth of trees, vines and shrubbery, dotted here and there with silk factories in the mulberry orchards, and flour mills on the river bank. The castle itself is a huge structure with an immense amount of massive masonry, but with very little of what we should call method and plan. Much energy has been wasted in modern times in excavations at various points within these ancient walls in the vain hope of discovering buried treasure. This passion exhibits itself in ill-directed efforts in connection with al-

most every site of ancient buildings throughout the country, and has occasionally led to discoveries of more value to the antiquarian than to the hungry seeker after gold. A find of another sort was made a few years ago in this castle at Amasia. A shepherd wandering with his goats among these ruins accidentally came across a store of salt, and soon began secretly bringing down a donkey load of it every night and selling it in the market place in the morning. As salt is a government monopoly in Turkey, this procedure soon led to suspicion and discovery. The poor shepherd was robbed of his stealthy gains, and the government officials took out several tons of salt, which had been stored there, in some long past age, as one item of provision against a siege, and then forgotten and buried out of sight for centuries.

This castle is provided with one of those deep sloping wells, with a perennial supply of delicious water, but rather far to fetch. From the mouth of this well a covered passage, partially subterranean, leads to a far distant portion of the castle thus securing an undisturbed supply of the precious fluid to all parts of the enclosure.

As a fortress this rock was long since abandoned, and yet the visitor will not fail to notice one rusty old cannon, bolstered up on broken trunnions, but evidently kept in readiness for use. This serves no longer a military but a religious or rather a social purpose, One month in the year the pious Moslem eats and drinks absolutely nothing during the day time—from the time in the morning when he can tell a black hair from a white one until the sun goes down. Consequently the moment of sunset acquires at that time of year a most important significance, and the correctness of everybody's watch is not to be relied upon. So in every town which can boast the facilities, a cannon is fired at the exact moment of sunset. Instantly the voice of the

call to prayer resounds from every minaret, and the excessively religious spend a few moments in the form of worship, but it is to be feared that their devotions are interrupted by thoughts of the savory dish into which their less devout companions have already plunged their impatient fingers.

At various points in and about this castle scraps of Greek inscriptions have been found, and one or two of considerable importance are cut into the face of the rock, but they are much defaced and serve little purpose but to excite the students' curiosity. Perhaps the most striking relics of antiquity in connection with this place are the very remarkable rock-cut tombs in the face of the bluff. The amount of labor expended on these is almost beyond computation! The rock is a hard limestone, and the means employed, so far as we can know, were simply the pick and the chisel, and yet they have carved out marvelous results. About two hundred feet up the smooth face of the rock there is a series of excavations which it is not easy to describe. Not content with hollowing out a great chamber and then ornamenting the front with columns, arches, etc., they have carried a broad gallery, the entire height of the tomb, all the way around and behind it, and a similar separating space overhead. Thus cutting out a huge cubical block some sixty feet high, and square, and separating it entirely from the great mass of rock, inside of which it still stands. Three or four such excavations, differing somewhat in dimensions and workmanship, but similar in general design, are connected together by a shelf or gallery cut right in the face of the rock, some twelve feet wide and ten feet high, with a low wall of the native rock left standing on the outer edge. This gallery follows the windings of the original face of the mountain, with here a flight of steps, and there a tunnel though a projecting spur of the rock.

With the means at the disposal of the ancients the amount of labor represented by this series of tombs is comparable to nothing but that which produced the pyramids of Egypt. But, like those marvels of constructive energy, they would seem to fail of their immediate object, for, while they may have been used for the purpose designed, they are now voiceless and tenantless, they commemorate no one, and only excite in the beholder a profound regret at the absolute waste of so much energy. Similar tombs, but of a smaller size and generally inferior workmanship, are found on other rocks in the neighborhood, and about a mile from the town, right down near the roadside, is one which will vie with the finest of them in every respect. The natives have given it the name of "mirror cavern," because the entire limestone exterior surface of the cube is polished to a such wonderful smoothness and lustre that it actually does reflect objects, like polished marble.

There is one other mysterious relic of antiquity, which, though not immediately connected with the castle, deserves a word of notice in this connection, as a similar relic of an enterprize involving vast expenditure of energy, and now wholly useless, though in this case originally with a very practical design.

The river Iris as it approaches the city runs through a fertile valley and very near to the precipitous base of a huge mountain, which rises right back of Amasia, called for some reason "guitar mountain," a very noted place for robbers. Between the river and the base of the cliff runs the great highway to Sivas and Bagdad, and alongside this road, in the very base of the rock are the remains of a large water-way cut right out of the solid limestone. Traces of this more or less distinct run along for miles. It is about four feet deep and the same in width, a continuous stone trough, originally covered with heavy slabs of stone, and evidently designed to

bring a fine supply of water into the city, though nothing indicates whence the supply was to come. The labor involved in this undertaking proves a populous city, a powerful government, and considerable skill in mechanical arts, but the questions who? when? and how? remain unanswered. Local tradition will never leave such a thing without an explanation and here the people tell a pathetic story of Ferhad, a young neighboring chieftain, noble, wealthy, brave and handsome, who fell in love with the belle of the city, daughter of the feudal lord. She agrees to accept him, but asks as her bridal gift that he conduct the water of his native village into the city. Nothing daunted, he heroically undertakes the work, and toils on forgetful of the passage of time in the constancy of his affection. At length his task approaches completion. He meets at the outskirts of the city an old woman who asks the object of his toil. He tells his simple tale, and shows a heart as young as ever. But she sadly informs him that his maiden true, past seventy years of age, had sunk into a hopeless tomb. He quits his undertaking, yet incomplete, dies of a broken heart, and is buried beside his love on the top of the mountain. This kind of folk-lore is very common among the natives of the country and helps to while away the long winter evenings; not always as chaste and simple as this specimen, and often excessively tedious and pointless, but sometimes spirited and witty and in rare instances embodying a truly valuable tradition.

If you will follow this same road for a day's journey, as it winds through the valleys in a southeastly direction, towards night as your intelligent steed begins to quicken his pace in anticipation of his evening meal, a sudden turn in the road spreads out before you a wide plain covered with rich vegetation. Right in the midst of it rises a high, abrupt and very picturesque rock,

crowned with another old castle, and with a large town clustering around its base. This is the town of Toorhal, one of the regular stations for changing post-horses, and the place for the finest melons that grow on the face of the earth. Here you may pray that your journey be not in the early spring, for then the river rises and respects no bounds, flooding the whole country. I have been two hours trying to make the last mile before reaching this town, with water most of the time above the saddle girths, and then had to swin my horse across the rapid stream in order to get to the island on which the town stood.

This castle repeats many of the characterists of the others, and needs no special comment. If you go ten miles to the southwest you will come to a similar town, with a similar rock and a similar castle, only that here there is a greater abundance of marble slabs with Greek inscriptions on them. These are mostly touch stones, and present many phases of life and views of death, some being heathen and some Christian. You will find them generally built into the walls of the castle or of some other building, with total disregard of position, and with no effort at defacement, except where the figure of the cross appeared. There you would see marks of determined and spiteful mutilation, failing, however, generally to remove traces of the original design. This is the town of Zela, which bore the same name two thousand years ago, and is celebrated in history as the spot where Julius Cæsar met and overcame the army of Pharnaces, and returned his brief and sententious report to the senate in the words, "I came, I saw, I conquered."

If instead of coming here we had traveled from Toorhal on the road southeast for another twenty-five miles, we should have come to the town of Tocat, where another old castle frowns upon us from an overhanging rock.

Clambering up this hill we find again a repetition of

the main points in other places,—the confused plan, accommodated largely to the shape of the rock,—the profusion of towers and heavy walls,—the deep well,—the single cannon retained for use during the Ramazan fast,—and the otherwise strikingly deserted and lonely look of things. But we are well repaid for our climb by the wonderfully varied and charming prospect on every side. The large city of Tocat nestles down at the foot of a high range of the Anti Taurus mountains, and even to this high and lonely spot comes up the deafening clangor of the copperplate beaters, for this is a great centre of the copper industry. Government mines far to the south send all the copper here to be remelted and cast in regular blocks for conveyance to the capital, and here are made all manner of copper vessels for all the country around. Across the ravine in front of us are the foundries with a small range of Scoriæ mountains behind them. And over on the skirts of the next hill is the Armenian cemetery, where was laid the precious dust of Henry Martyn, pioneer missionary and Bible translator in India and Persia, who died here while traveling toward his native land in 1812. Yonder, toward the north, stretches a winding valley full of rich verdure. A day's journey in that direction would bring you to the town of Niksar, the Turkish corruption of Neo-Cæsarea, a town not unknown in ancient history, and at present a Greek episcopal see. Here too we find an ancient castle with interesting inscriptions, and noted for possessing several large guns bearing the name of Abd-ul-Hamid I., namesake of the present Sultan, who reigned just a century ago.

To the east of this place is Kara Hissar, or the Black castle, a large town which takes its name from the very striking castle on the summit of a high black basaltic rock. And so on we might go through the whole country, finding ruins and castles and inscriptions every-

where, and an excellent opportunity to study ancient history, as well as modern humanity.

But it merely remains to mention in brief a very wonderful group of castles and other ruins at a place called Boghaz Keny, near Yozgat. They are of a totally different character from those I have spoken of. Huge blocks of stone, cut with greatest care but put together in some parts after the most primitive principles of cyclopean architecture,—are large and well-planned temples with extensive enclosures,—long and very elaborate underground passages, constructed with incalculable pains,—high square towers composed of immense blocks of fine grained limestone; and, most striking of all, a very extensive set of sculptures on the face of a series of rocks, including a great variety of figures of men and beasts and birds, and many symbolic marks, in one place an inscription in ten lines, thirty-four feet long, but so injured by time as to be wholly beyond the reach of deciphering. These very strange remains have been a mystery to all observers for many centuries, until now within a few years,—I had almost said months,—they have been identified as doubtless the work of the strange old Hittite kingdom. But all effort thus far to read the language or interpret the symbols has been unavailing, and this remains for future discovery.

It is needless to attempt to gather up any conclusions from such a ramble among the castles and ruins of Asia Minor. To one who has the privilege of visiting these historic sites, history becomes a new thing. Its facts and scenes become living realities, and every word which is subsequently read, has an added force and meaning. There is, moreover, a broad and fresh field for original investigation, which awaits the patient labors of some historic genius. Its treasures will some day be added to our accumulating stores, and new light will doubtless

fall on many a page of ancient lore, with perchance the breaking down of some cherished idols, and the bursting of some fantastic bubbles. But whatever is done in these lines ought to be done quickly, for the ruthless hand of time, and the still more remorseless vandalism of the Turk, leave every year less and less of the picturesque marvels of the ruined castles in Asia Minor.

APRIL 5, 1887—THIRTY-SEVENTH REGULAR MEETING.

William G. Stevenson, M.D., president in the chair; fifteen members and sixty guests present.

Messrs. Herman Zingg, Charles C. Mills and W. J. Bolton were elected active members.

The Rev. H. L. Ziegenfuss gave an address entitled "Some Physical Processes and Concomitants of Mentation." The paper was discussed by Messrs. Stevenson, Nilan, Bartlett, Van Gieson, Swan, Winne, and Cooley.

MAY 3, 1887—SIXTH ANNUAL MEETING.

William G. Stevenson, M.D., president in the chair; fifteen members present.

Mr. Edward Elsworth, treasurer, rendered a full report of receipts and expenditures for the fiscal year ending May 3, 1887, of which the following is an abstract:

Balance in treasury May 4, 1886	$809 29
Total receipts during the year	2,448 95
Total	$3,258 24
Total disbursements during the year	1,120 02
Balance in treasury May 3, 1887	$2,138 22

Dr. Stevenson, from the committee on museum and library, gave an itemized report of expenditures for the museum and library amounting to $191.19.

The president, Dr. Stevenson, gave the following report:

Members of Vassar Brothers Institute: By an amended rule of the society, the duty of making the annual report has been transferred from the secretary to the president, and in obedience thereto, I have the honor of briefly speaking in relation to the work which has been done by the Institute and its sections during the year ending this date.

During the year one member—Dr. John R. Cooper—has died, and the names of two members have been taken from the roll by resignation.

Six members have been elected. The present membership is one hundred ten.

As indicated by the treasurer's report, the finances of the society are in a satisfactory condition.

By an amendment to article VIII, of the by-laws, the museum and the library have been placed under the direct charge of the Scientific Section, and the curator of the museum and the librarian have been made officers of this section

The following addresses were given before the Institute during the season of 1886–1887 :

1886.

November 9. "Genius and Mental Disease."
Wm. G. Stevenson, M.D., president of the Institute.

1887.

January 4. "Pre-historic Man in America."
James M. DeGarmo, Ph. D.

February 1. "Ruined Castles in Asia Minor."
Rev. Edward Riggs.

April 5. "Some Physical Processes and Concomitants of Mentation."....Rev. H. L. Ziegenfuss.

The average attendance of members and guests at these meetings was one hundred seventy-five.

The publication of Volume IV, which has been delayed, will soon be issued, and will contain the transactions of the Institute and of the Scientific Section, from May 5, 1885, to May 3, 1887.

SCIENTIFIC SECTION.

The excellent order of work which has at all times characterized the Scientific Section, has been steadily maintained, and the general interest in its meetings has been increased.

The following papers were read before this section during the season of 1886–1887:

1886.

December 1. "Earthquakes."
Wm. G. Stevenson, M. D., chairman of the Scientific Section.

December 15. "Recent Discoveries of Fossiliferous Cambrian Strata in Dutchess County."....Prof. William B. Dwight.

1887.

January 12. "From Coal-tar to the Alizarine Dyes."
LeRoy C. Cooley, Ph. D.

February 1. "Iodine blowpiping."....Mr. Charles L. Bristol.

February 23. "Bacteria."....Miss Isabel Mulford.

March 9. "New Stars in Andromeda and Orion."
Miss Mary W. Whitney.

April 6. "Continental Evolution."...Charles B. Warring, Ph. D.

April 20. "The Cutting at Croton Point, N. Y."
Charles B. Warring, Ph. D.

One hundred twenty-three volumes of books, and one hundred seventy-seven pamphlets have been received for the library; and in addition to the names given on page 54 of this volume, the following societies, journals and state departments, have been added to our "exchange list," viz:

American Institute of Mining Engineers.
Science Observer.
Colorado Scientific Society, Denver, Col.
Franklin Institute.
Geological Survey, Pennsylvania.
Canadian Institute.
Johns Hopkin's University.

Many valuable books are now in the library, and the hope is expressed that under the care of the Scientific Section a more earnest effort will be made to catalogue these books, and to prepare rules for their use, that greater benefit may be derived therefrom.

The museum has received many valuable specimens by donation and by purchase, of which a detailed report is given by the curator.

At the annual meeting of the section held April 20, 1887, the following officers were elected :

Chairman, . . . Mr. Charles N. Arnold.
Recording Secretary, . Mr. James Winne.
Curator of the Museum, . Prof. Wm. B. Dwight.
Librarian, . . . Mr. Charles L. Bristol.

LITERARY SECTION.

The following papers and discussions were given before the Literary Section during the season of 1886–1887 :

1886.

November 16, "Character and its Enemies."
Mr. L. F. Gardner, chairman of the Literary Section.
November 23. "Animal Intelligence."....Mr. Edward Burgess.
November 30. "A Scientific Basis for Religion."...Mr. Wm. C. Albro.
December 14. "Bad Citizenship."....Mr. Henry V. Pelton.
December 21. "Russia."
1 The expediency of allowing a sea port in the South.
Mr. W. C. Albro.
2 Russia's place among Nations..Mr. Edward Burgess.

1887.

January 11. "The three R's."....Mr. Isaac Tompkins.
January 18. "Theodore Parker."....Mr. L. F. Gardner.
January 25. "Bulgaria."....Mr. Edward Elsworth.
February 8. "The Mugwump."....Rev. Wayland Spaulding.
February 15. "The Danger of Great Wealth" (Discussion).
Messrs. Gardner, Bartlett and Burgess.
February 22. "The Monroe Doctrine."....Mr. Charles B. Herrick.
March 8. "The Puritans and Roger Williams."..Mr. Cyrus Swan.
March 15. "Art."....Mr. George Bissell.
March 22. "Africa," (Discussion).
Messrs. Gardner, Burgess and Bristol.
March 29. "Incidents of Travel."....James Nilan, D.D.

At the annual meeting of this section Mr. L. F. Gardner was re-elected chairman and Dr. L. V. Cortelyou was re-elected recording secretary.

Because of the expense involved in opening a studio for instruction in drawing, painting, modeling, and the

industrial arts, the Art Section very properly desired information in relation to the patronage which such a school would receive before it incurred expense therefor.

Since, however, its solicitations have met with no favorable response, the section is justified in still further postponing any effort to organize its work.

In closing the sixth year of its corporate life, the Institute may well feel a reasonable pride in the work it has accomplished.

Future success, however, should not be made dependent on past memories or works, but upon renewed purpose and increased energy in prosecuting the work which pertains to us as members of the society.

In this conection, you will pardon the suggestion that hereafter all popular, public addresses be under the immediate charge of the Institute, and that the sections concentrate their energy to critical, technical studies and discussions. This aims at individual improvement—by which knowledge is promoted—which is the first essential object of our society!

The Literary Section would, under this plan, study and discuss literature and history—not as heretofore in a popular way—but critically, for the mere sake of knowledge. This method has proved eminently satisfactory in the Scientific Section, and it is commended to the Literary Section as worthy of an earnest trial.

In addition to the many interesting subjects which have so properly occupied the attention of the Scientific Section, there are still other questions relating to the health and comfort of our citizens—such as sewerage, drinking water, ice, and illuminating gas—which should command attention also. In order that such subjects may be duly investigated, it is suggested that the section appoint special committees to examine them in all their sanitary bearings, and to make a detailed report thereof to the section. Such reports will be of perma-

nent value to the community, and will be highly creditable to the section.

In closing my official duties, it remains for me to express my cordial thanks to you, fellow members, for the courtesy and consideration you have at all times extended to me.

Respectfully submitted,
WILLIAM G. STEVENSON,
President.

The following gentlemen were elected trustees of the Institute for 1887–1888 :

JOHN GUY VASSAR,	EDWARD ELSWORTH,
S. M. BUCKINGHAM,	WILLIAM G. STEVENSON,
WILLIAM B. DWIGHT,	LEROY C. COOLEY,
CHARLES N. ARNOLD,	A. P. VAN GIESON,
HENRY V. PELTON,	WILLIAM T. REYNOLDS,
CHARLES B. HERRICK,	EVAN R. WILLIAMS.

The following gentlemen were elected officers of the Institute for 1887–1888 :

President, . . .	A. P. VAN GIESON, D.D.
Vice-President, . .	MR. HENRY V. PELTON.
Secretary, . . .	MR. CHARLES L. BRISTOL.
Treasurer, . . .	MR. EDWARD ELSWORTH.

TRANSACTIONS

OF THE

SCIENTIFIC SECTION

OF

VASSAR BROTHERS INSTITUTE.

1885–1887.

VOL. IV. PART II.

OFFICERS.

1885–1886.

Charles B. Warring, Ph.D.,	*Chairman.*
Mr. Charles N. Arnold,	*Recording Secretary.*

1886–1887.

William G. Stevenson, M. D.,	*Chairman.*
Mr. Charles N. Arnold,	*Recording Secretary.*

TRANSACTIONS

OF THE

SCIENTIFIC SECTION.

NOVEMBER 18, 1885—THIRTY-NINTH REGULAR MEETING.

Eight members and twenty-five guests present.

THE QUICHÉ STORY OF CREATION.

AN ADDRESS DELIVERED BEFORE THE SCIENTIFIC SECTION.

BY CHARLES B. WARRING, Ph. D., CHAIRMAN.

My study of the Hebrew cosmogony, has led me to examine the claims of the Chaldean story to be the source from which it was obtained. A careful examination of the internal evidence has led me to the conclusion that this claim has no real foundation.

When reading the third volume of Bancroft's great work, *The Pacific Races*, my attention was directed to the Quiché account of creation, when, to my surprise, I found greater and much more numerous resemblances to the Hebrew story than were presented by the Chaldean myth. These were not such as would strike the reader at first glance—the opposite was true—but were in the underlying thought and in the order of the two narratives.

This is the cause and the apology for bringing the Quiché cosmogony before the section.

Among the nations to the southeast of Mexico against whom the Spaniards turned their arms after the conquest of that empire, was a people called the Quichés,

or the Quiché Indians. Their religion was a gross polytheism, and their gods were monsters of cruelty.

There was current among them a story of the making of the world. It was preserved, after their manner of writing by pictures, in a book called the *Popol Vuh*. This book is not now in existence, as it has not been seen since the conquest. Some years after that event, how many I do not know, certain of the Quichés gathered up the traditions of their people, this story of creation among others, and wrote them out with Roman letters, but in their ancient language.

About the year 1720, this collection was translated into Spanish by Father Francisco Ximines, a Dominican of great repute for his learning and love of truth. His position as curate of an Indian town in Guatemala probably brought the book to his knowledge and incited him to undertake the labor of translation. The manuscript lay a long time without attracting attention, but in 1856 was brought to the notice of European scholars by a paper read by Dr. Schlerzer before the Vienna Academy of Sciences.

Dr. Schlerzer found the MS. in a convent of the Dominicans in Guatemala. It contained the Quiché text and the Spanish curate's translation. The Abbé Brasseur de Bourbourg was dissattisfied with Ximines' translation, so in 1860, he settled himself among the Quichés, and by the help of natives joined to his own practical knowledge of their language, elaborated a new and literal translation. These facts account for a tinge of biblical expression which appears in portions of the narrative.

In giving the Quiché story I shall omit much of the verbiage, and shall condense a good deal, without, however, intentionally changing the sense.

It begins thus: "And the heaven was formed, and all the signs thereof fixed by the Creator and Former—

he by whom all move and breathe, he whose wisdom has projected (planned) the excellence of all that is on the earth, or in the lakes, or in the seas."

Here is a real parallelism to the opening verses of Genesis. It is God that made the heaven and earth. The Bible says: "In the beginning God created the heavens and the earth." The Chaldean says: "The heaven and earth and sea preceded the gods. Then the great gods were not made, any one of them, and order did not exist. Kar and Kisar were made first, and after them the lesser gods." The Chaldean teaches the eternity of the material universe. The Quiché agrees with the Hebrew in placing an intelligent being before matter, and says that he was the creator of all things. The Chaldean makes "the sea the producing mother of all things."

The Quiché account goes on: "There was as yet no man nor any animal, nor any green herb, nor any tree; nothing was but the firmament."

The Hebrew says the earth was void, and places the firmament before the making of plants and animals.

The Quiché story continues: "The face of the earth had not yet appeared—only the peaceful sea, and all the space of heaven. All was silent. Nothing existed but the quiet waters, nothing but immobility and silence in darkness and night."

In the next two paragraphs are introduced gods many, who speak to one another, and meditate. This speaking to one another and mutually taking counsel, is like the Hebrew narrative where God is represented as speaking, planning, and approving, and, in this last and grandest act of creatorship, as taking counsel of the Triune Godhead. "Let us make man in our image and in our likeness."

In the Chaldean Genesis, the gods are dumb, they neither plan, nor purpose, nor speak. Like the forces

of nature, they put forth their power without announcement of purpose, or words of approval.

The Quiché Genesis continues: "And the creation of the earth was after this wise: 'Earth,' they said, and on the instant it was formed. Then the mountains rose above the waters; in an instant the mountains and the plains were visible, and the cypress and the pine appeared.

"After this the earth was peopled with the various forms of animal life. On these the maker called 'to speak our name, honor us as your mother and father.' But the animals could not speak, they could only cluck and croak, each after his kind in a different way."

This displeased the creators and they said to the animals: "Inasmuch as ye cannot praise us, neither call upon our names, ye shall be killed and eaten."

The account then relates that the gods took counsel together and determined to make man. So they made the first man of clay.

This, it seems, was a very imperfect and unsatisfactory man; so he was consumed in the waters.

Again there was counsel in heaven; Let us make an intelligent being who shall adore and invoke us. So they made a man of wood and a woman of pith. The result in this case also was unsatisfactory. "They peopled the earth with sons and daughters, but they forgot their maker; they led useless lives; they lived as beasts live; they forgot the Heart of Heaven, *i.e.*, their creator.

"Then was the Heart of Heaven wroth, and destroyed those ingrates with terrible destruction. He rained day and night upon them a thick resin. They went mad with terror. They tried to mount upon the roofs of their houses, and the houses fell; they tried to climb the trees, and the trees shook them off far from their branches. They tried to hide in caves and dens of the

earth, but the caves and dens closed themselves against them. Their eyes were torn out; their heads were cut off; their flesh was devoured. Thus all were destroyed but a few, and these now live in the woods as little apes."

It would seem that in the long series of generations by whom these traditions were handed down, the story of creation, and the story of mankind's being overwhelmed with destruction because they had forgotten their creator and led useless lives, and lived as the beasts live, had come to be considered as parts of one account.

After this destruction the gods made four perfect men (the number seems to be a reminiscence of the number of men that peopled anew the world after the deluge,) of yellow and white maize. They had neither father nor mother. Their coming into existence was a miracle wrought directly by the creator. "Verily at last there were found men worthy of their origin and destiny. Grand of countenance and broad of limb, they stood up under the rays of the morning star—sole light as yet of the world—they stood up and looked. They saw the woods and the rocks, the lakes and the sea, the mountains and the valleys, and the heavens that were above them. Then they returned thanks to those that had made the world and all that was therein.

"But the gods were not wholly pleased. There was counsel again in heaven: 'What shall we do with man now? It is not good this that we see; these are as gods; they would make themselves equal with us; lo, they know all things great and small.'"

Here are evident the marred and distorted features of the mysterious scene in the Garden of Eden. "Ye shall become as gods, knowing good and evil."

Perplexed at the prospect of men becoming so wise, the gods endeavor to prevent it, and for that purpose they say: "Let us contract their sight. Thereupon the

Heart of Heaven breathed a cloud over the eyes of men, and a veil came over them as when one breathes on the face of a mirror; thus their eyes were darkened; and that which was afar off was not clear to them any more, but only that which was near."

The myth then relates that these four men slept; and that the gods took counsel, and made four women, one for each of the men.

This creation of women in man's sleep, seems to be confounded with the four wives of our second great progenitors—Noah and his three sons.

The myth adds: "Now the women were exceedingly fair to look upon, and when the men awoke they were glad because of the women."

This is all of the *Popol Vuh* that pertains to the creation.

The remainder of the narrative is a series of myths relating to the wanderings of these primeval men, the introduction of idol worship and the offering of human sacrifices, but casting no light upon either.

The following is a brief statement of parallelisms, or resemblances, between the Quiché account and the Mosaic, at the same time noting the agreements, or rather the lack of agreements, with the Chaldean story deciphered by Mr. George Smith and others.

1. Both the Quiché and the Mosaic account say that God preceded and formed the universe. The Chaldean puts the universe first. In the former, God is creator, in the latter the sea is the creator of the gods; they merely introduce order.

2. Both Quiché and Hebrew speak of the earth as void.

3. Both say that darkness covered all things. The Chaldean inscriptions do not speak of darkness.

4. The former speak of a firmament, the Chaldean does not.

5. The Quiché and the Hebrew both represent the deity as speaking. Not so the Chaldean. Its gods are silent.

6. Both Hebrew and Quiché place next in order the rising of the land from beneath the waters. Chaldean tablets, so far as known, say nothing about the land rising out of the water.

7. The Quiché story places next in order vegetation; and so does the Hebrew. The Chaldean, so far as we have it, says nothing of the production of vegetation. After this there is a variation in the order. Moses places the fiat in reference to the lights, after the appearance of vegetation, and before the appearance of man; but the Quiché account represents man as made before the sun. The Chaldean is silent.

8. Animals are made next after trees and plants.

9. Both Quiché and Hebrew place the creation of man after the appearance of certain vegetation and of certain animals. The Chaldean is silent.

10. Lastly, both Quiché and Hebrew agree that God made woman after he had made man.

11. That she was made while man was in a profound sleep.

12. And that there was one wife for one man.

Here are twelve particulars in reference to which the Hebrew and the Quiché accounts coincide.

The agreement between the Bible account of creation, and the Chaldean tablets so far as they have yet been found, is confined to the following few and unimportant particulars:

1. Both near the opening of the story speak of "waters."[1]

2. Both Hebrew and Chaldean speak of sun and moon,

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1 See Lenormant's version in his Beginnings of History, page 491. Compare with Genesis the next two tablets (pp. 491, 492, 493), and note the absence of agreement.

but what they say is totally different in form and spirit.[1]

3. Both say that God (in the Hebrew account), or "the gods" (in the Chaldean) made cattle, beasts, and creeping things.[2]

It is difficult to see how there could be less agreement between any two accounts purporting to speak of the earth and its inhabitants.

If we must choose either the Quiché or the Chaldean as the source whence the Hebrews derived their cosmogony, the internal evidence is greatly in favor of the Quiché.

Not even the boldest advocate of the human origin of the Mosaic account has thought that it came from the Quichés, and if we may dismiss that, *a fortiori* we may dismiss the claim for the Chaldean inscriptions.

Letting all this pass, the question arises as to the origin of this Quiché story.

The many points of resemblance to the account in Genesis, and the fact that the earliest written record we have of it, came into existence a number of years subsequent to the Spanish conquest, suggest that the story is a corrupted version of the Bible account.

But to this easy solution there are insuperable objections. It seems incredible that the Quichés, unless Christianized, would accept the Bible story from the hated hands of their conquerors and oppressors. On the other hand, converted Indians would not have piled on the Bible account, such an enormous amount of polytheistic myths; and those Indians who remained attached to their ancient religion, would not have accepted it at all. We, therefore, must look for some other origin for this story.

Does not the possession of an account so resembling

1. Idem; page 495.
2. Idem; page 497.

that found in the sacred writings of the Hebrews add another to the proofs that the Indians of Central America had, at some time in the remote past, close connection with the Hebrews? The great length of time and the growth of superstition would account for the thick incrustation of fable with which the account is overlaid.

The only reasonable conclusion, so far as I can see, is that at some remote period the ancestors of these people received by tradition the same story which is found in our Bibles. This period probably antedates the Chaldean inscriptions, or at least the Quiché tradition did not pass through Chaldean hands, since, except in their polytheistic character, there is no resemblance between them.

If this theory be rejected, then the source of this account remains a curious problem, important in its bearing upon the question of the origin of the American races.

The possession of such a cosmogony adds another link to the chain of evidence that goes to prove the unity of mankind.

DECEMBER 2, 1885—FORTIETH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

Prof. William B. Dwight gave a talk on the formation of "Geodes."

DECEMBER 16, 1885—FORTY-FIRST REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; ten members and thirty guests present.

LeRoy C. Cooley, Ph.D., gave an interesting address on "The Periodic Law of the Elements," and it is regretted that no manuscript of the address has been, as yet, prepared for publication.

JANUARY 13, 1886—FORTY-SECOND REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

F. Monteser, Ph.D., read a paper on "The Axioms of Geometry," of which the following is a brief abstract:

He discussed the question whether the postulates which form the basis of our geometry are independent of experience or not. Kant assumed that they are, and made that assumption the cornerstone of his theory of knowledge *a priori*. His argument was that the axioms of geometry have a character of true generality and necessity which no experience could possibly give to its results. Any assumption, contrary to the axioms, according to him, is not only false, but essentially inconceivable. Modern mathematical investigations have overthrown that argument and taught us the real meaning of the axioms. It was shown by the researches of Gauss, Lobatschewsky, Beltrami, and especially Riemann and Helmholtz, how much of experimental facts there is really implied in those apparently so simple propositions, and how, starting from suppositions, contrary to Euclid's axioms, one could construct a perfectly legitimate and logically consistent system of non-Euclidian geometry. In other words, it was proved that our space is only a special form out of an infinite number of possible spaces, and that more extensive measurements are needed before we can decidedly say that this actually existing space really has those properties which we are accustomed to ascribe to it. Or to put the question in a very simple and practical form, we may state our point thus: If we have two straight lines, perpendicular to the same straight line, we know that they will not intersect—as far as we can prolong them. But we are not at all sure whether they would intersect or not when prolonged to a distance, compared with which the diameter of the earth's orbit is exceedingly small. In this way geometry loses its trans-

cendental character and becomes a physical science. Its postulates "are not necessary and universal truths, but merely axioms based on our experience of a certain limited region. Just as in any branch of physical inquiry we start by making experiments, and basing on our experiments a set of axioms, which form the foundation of an exact science, so in geometry our axioms are really, although less obviously, the result of experience." (*Clifford, Common Sense of the Exact Sciences*, Chapt. IV, § 19.)

After a prolonged discussion of this interesting paper, LeRoy C. Cooley, Ph.D., read the following note relative to the

"DECOMPOSITION OF THE 'ELEMENT' DIDYMIUM."

Dr. C. A. Von Welsback, has lately (June 18, 1885) sent a paper to the Vienna academy announcing the decomposition of the so-called element didymium. (See *Chemical News*, vol. 52, and *Nature*, vol. 32, p. 435.) He secured this result by treating the double nitrate of didymium and ammonium with a salt of lanthanium. The two constituents differ by well marked characteristics. They have different atomic weights. The salts of one are green, of the other amethyst red. Each has a characteristic line spectrum and by mixing the two substances in proper proportions the product yields the spectrum of didymium.

The decomposition of an "element" is in itself a result of great importance ; the decomposition of an element by the ordinary chemical processes of the laboratory is a result of still greater interest and importance, because, in addition to the fact of the compound nature of the element, it proves that new methods of research may not be needed in order to decompose them, and should incite chemists to continue their industrious use

of the means already in their hands for similar work on other members of this class of bodies.

JANUARY 27, 1886—FORTY-THIRD REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

The following paper was read:

"PRIMORDIAL ROCKS OF THE WAPPINGER VALLEY LIMESTONES."

BY PROF. WILLIAM B. DWIGHT.

The presence of rocks of the Potsdam group in association with the limestones and shales of Dutchess county, N. Y., has long been suspected on stratigraphic grounds, but until the present time this fact has never been proved by positive paleontological evidence.

At the bases of Fishkill and Stissing mountains, a thick stratum of quartzyte is found between the underlying Archæan of those mountains, and the overlying limestones, now known to be respectively Calciferous and Trenton. This quartzyte shows planes of stratification, and is conformable to the overlying strata of limestone. Obviously, by its stratigraphic position, it appears to represent the Potsdam group, and this assignment has been made for it, in the localities mentioned, by Prof. Mather, Prof. J. D. Dana and others.

Notwithstanding that considerable search has been made, no fossils have yet been found in this quartzyte stratum, by which this reasonable hypothesis might be established. I am not aware that any geologist has heretofore found reason to suspect the presence of a Primordial stratum among the limestones of the region, and certainly I have had no such expectations myself. The observations made during the last few years in the Wappinger valley (or "Barnegat") limestones, have definitely proved them to be composed extensively and continuously of conformable strata of the Calciferous

and Trenton groups. In carrying out the work, in which I have for several years been engaged, of preparing a detailed stratigraphical chart, on April 25th of the present year, to my great surprise, I struck a ledge of rock in the Wappinger valley limestone belt, which proved rich in Potsdam fossils.

This remarkable locality is on an estate owned by Mr. Albert K. Smiley, proprietor of the Lake Mohonk house. It is in the outskirts of the city of Poughkeepsie, to the southeast. It is about one mile southwest of Vassar college, eight hundred fifty feet south of the southwest corner of the driving-park on Hooker avenue, and about two thousand two hundred feet west of the road which passes south along the east side of the same.

The Potsdam rocks are found in a series of low hills or ridges trending northeasterly and southwesterly in parallel lines. The most interesting paleontological features appear to be concentrated in a ridge lying at the northeast corner of the group (hill A, plate VI). This is situated immediately to the southwest of Smiley's detached barn, which is itself south of the southwest corner of the driving-park. This hill is about three hundred feet wide and one thousand four hundred feet long; it is mostly covered with soil, the rock cropping out on each side, but chiefly on the east, in a few narrow, quite inconspicuous ledges; it would never have attracted my attention but for its occurring within the range of a systematic survey.

Lithologically this Potsdam rock is exceedingly variable, and all the varieties described occur within the small compass of a few acres of ground. It is everywhere (so far as already examined) calcareous, all its varieties effervescing with acids. It is also everywhere more or less arenaceous; often conspicuously such. Large portions of it are a tough, compact limestone, often quite dark, and frequently filled with a conspicuous

fucoid-like tracery; the weathered surfaces are often rough with sand-grains. This variety passes into one which differs chiefly in being fissile into more or less thin slabs; this variety often alternates with layers of exceedingly thin and friable shale, the folia of which are covered with loose sand as they decompose.

The rock also passes on the one hand into argillaceous varieties represented by a smooth, fine-grained, massive argillaceous limestone well exhibited on the west side of the main fossiliferous hill; and represented also by a very fissile and smooth calcareous shale, well shown in a hill (E) which is a continuation, in the second field south, of the above-mentioned hill. Although nearly as fissile, this variety differs essentially in appearance from the closely adjoining Hudson river shales, which are much darker, and more glazed or shining on their surfaces. On the other hand, this Potsdam rock passes into a very solid, massive quartzyte, the fine sand-grains of which are everywhere in absolute contact, so that in appearance it is only a grade less compact than the quartzyte of Stissing mountain; but the minute interstices are occupied by calcareous material, so that the rock effervesces a little under acids.

In many places, again, the material takes the form of brecciated conglomerate; this is well shown in the western Potsdam hill at the northern extremity of the tract under consideration (hill B), and its extension south into the next field (hill D); also in the western marginal Potsdam ridge just south of T. A. Hinkle's house in the next field.

The southeastern extremity or fork of the main fossiliferous hill (A) is a solid mass of peculiar oölyte, made up of spherules which are simply aggregations of rhomboidal calcite crystals imbedded in the interstitial mass of quartzyte or calcareous quartzyte. I have found pebbles of this peculiar oölyte in the conglomerate

above mentioned in the ridges of the western margin, suggesting a later date of deposition for the latter. In many places these Potsdam rocks exhibit very distinctly oblique lamination.

The strike and the dip are quite variable. The most general strike of these Primordial strata appears to be about N. 21° E. (true), and the most general dip about 55° easterly. Some further statements as to local variations will be given subsequently.

In order to determine the true stratigraphic relations of these Potsdam and related strata, I have made a special detailed examination of a district covering about a mile and a half square in the vicinity of this locality, and embracing about a mile and a quarter of longitudinal extension in the direction of the strike. This district is mapped in plate I.

This Potsdam rock is one of the component members of the most western, and by far the broadest of three parallel belts of the Wappinger valley limestones, which, in this vicinity, rise between the Hudson river shales. In general, all these limestones and shales lie in a series of abraded folds, having usually a conformable strike of from N. 20° E. to N. 30° E. These folds are closely compressed, and pushed over to the west, so that the earlier limestones usually overlie by inversion the later shales lying to the west of them.

The particular belt of limestone which stretches southwest from the driving park is about three thousand five hundred feet in width at its northern extremity, and about six thousand feet wide at the southern portion of the limited district now under consideration. This southern transverse line of six thousand feet passes through the middle of the estate of W. S. Johnston on the Albany post road.

This belt terminates toward the north, quite abruptly, along an almost straight line which cuts it apparently

at a right angle to the strike. This line runs along the southern margin of the driving-park, continues east through the fields parallel to Hooker avenue till it meets the Hudson river shale in the meadows of Casper creek. Immediately north of the driving-park, in fields on the north side of Hooker avenue (locality O, plate I), in place of limestone there are hills of Hudson river shale, and this same rock crops out along Hooker avenue to the east all the way to Casper creek and beyond. From this line these shales continue unbroken many miles to the north beyond Hyde Park. Along this northeasterly and southwesterly line, the intervening space between the outcrops of shale on the north and of limestones on the south consists of a mostly level strip of a drift having bogs and springs along its southern edge. Evidently this is a line of fault, across the strike, between the Hudson river shales on the north and the Potsdam, with its associated Calciferous and Trenton, on the south. This trough, cut through the hilly ridges, is a conspicuous feature in the local landscape. It is made available for traffic by the avenue that passes through it obliquely into the city; it furnishes the long and smooth plat (mostly underlain by the shales) which constitutes the driving-park.

The stratigraphic relations of the western margin of the belt of Potsdam have proved far less obvious than those of its northern extremity. But after a careful, detailed inspection of the ground, a correct solution, as I believe, has at last been reached. During the earlier examinations, Calciferous or Trenton strata were naturally looked for, in intervening folds, between the Potsdam and the Hudson river shales to the west. The western hill of conglomerate above mentioned (hill B) and its southern extensions, were at first taken to be the Trenton coralline conglomerate which occurs abundantly in the vicinity. But on further investigation, two

facts were developed which opposed this supposition. The first fact was that the lithological constitution differed entirely from that of the Trenton wholly calcareous conglomerate, in being highly arenaceous, as also in the entire lack of the microscopic corals which abound in the Trenton. On the other hand, its lithological characters are quite in harmony with those of the adjoining ledges known to be Potsdam.

In the second place, an outcrop of the Hudson river shales was discovered close to the western base of this hill, within a few feet, thus marking the line as the actual western limit of the limestone. This limit is at the most but three hundred feet west of the fossiliferous Potsdam strata, a space by no means sufficient to allow the presence of Calciferous and Trenton strata of the usual thickness exhibited in the region. A further study of all the phenomena makes it quite certain that the Potsdam extends to the western margin of this belt of limestone, in its entire southern extension through the district examined. This limestone margin (as traced from the north) is broken by three or four jogs, as the belt widens to the west, until the field next south of T. A. Hinkle's cottage is reached; from that point it is nearly straight. Close to this line, at the distance of a few feet throughout its entire length, there are many outcrops of Hudson river shale which continue west to the Hudson river, unless Utica shales may occur at some points. The shales are in many places separated from the Potsdam limestone by lines of springs and ponds, or by dry gullies. From the extreme northern end, the plane of contact is marked by a line of ponds nearly to Hinkle's house. Close to this house, to the west of it, there is a deep gully, with the limestone on which the house stands, on the east, and the shales, standing nearly vertical in a bold exposure, on the west. This gully continues to mark the line to the southwest

as far as the turn in the Ferris road, beyond which the demarcation is produced by a different but no less striking plan; for south of this point the limestone remains a high conspicuous ridge, abruptly steep on the western side, while the shales, mostly covered by drift, form a level plain at its base.

These facts make it evident that there is also a line of fault between the Potsdam and the Hudson river shales at the western margin of the limestone belt, more or less parallel to the strike. The general direction of this line of fault is about N. 40° E. The Potsdam strata near the line of contact are generally deflected in such a way as to have less easting in the strike, which is in such places from N. 4° E. to N. 11° E. Thus at the extreme north end of the main fossiliferous hill (A) the strike varies between the limits just given, while the dip becomes as low as 20°. At the hill of calcareous quartzyte in the third field south of the driving park (hill F) the strike is in some places N. 11° E., and the dip 35°.

On the other hand, the Hudson river shales incline to acquire more easting in the strike, in the vicinity of the plane of contact. In such cases they have quite frequently a strike of N. 46° E. (their general strike in the vicinity being about N. 31° E). They also, in such positions, are often found inclined at a very high angle. These phenomena seem to indicate considerable friction by a lateral motion in a northeasterly and southwesterly direction, as well as in the upward motion, at the time of the uplift of the Potsdam. This fault is the more interesting, because it is evidently related to the great fault described by Sir William Logan and Prof. James Hall as extending from Quebec to the Hudson river near Rhinebeck. If, as has been suggested by Prof. J. D. Dana, this should more properly be regarded as a series of more or less parallel faults, the present

one would constitute the most southern one of the series yet described.

With regard to the eastern boundary line of this strip of Potsdam nothing definite can be determined from present data. There are no fossils to indicate the points of transition to the Calciferous and Trenton, which doubtless lie in abundant development to the east. The uncertainty is increased by the fact that a great part of the rock is deeply buried under drift. Thus, at the northern extremity, east of the fossiliferous Potsdam, after about six hundred feet of strata (which are probably to some extent at least of the same group), the limestone entirely disappears under a large hill of more than fifty feet depth of drift in A. Vanderberg's farm. (This hill is so covered with debris of Hudson river shale that one might readily suppose it to be the rock in place.) The limestone does not reappear until the southeastern base of Richmond hill on Casper creek is reached. That this eastern margin of the belt is Trenton is proved by a distinct outcrop of fossiliferous Trenton filled with *Solenopora compacta* ("*Chætetes compacta*") appearing in a small patch on a hillside five hundred twenty feet from the house of R. J. Kimlin on a course of N. 101° E. The Calciferous is doubtless extensively represented between the Trenton and the Potsdam, but this has not been paleontologically determined.

In view of the above facts, I can make only the general statement that the minimum width of the belt of Potsdam strata, measured on the surface of the ground, is somewhat over six hundred feet. If, as seems probable, there is at least one compressed fold, the actual thickness of the deposit must be over three hundred feet.

It is my present impression that the Potsdam folds occupy, as indicated on the accompanying map, a strip of about one thousand four hundred feet in width, and that the high and continuous limestone hill (K) on the

east of this strip may form the western margin of the Calciferous strata. In this case the small quarry in the field of W. S. Johnston, at the southeastern portion of the district (H), would be near the eastern edge of the Potsdam. This view is strengthened by the fact that lines of ponds and springs exist conspicuously along the base of this long eastern hill, indicating a possible break or slip between strata along this line. But as fossils have not yet been found in the last-mentioned hill, nothing more positive can be asserted in this connection at present.

A brief description may now be given of the paleontological features of these strata.

In the fossiliferous hill above described (A), organic remains are found chiefly on the eastern side near the summit in the southern half, though not in the oölytic portion. They occur both in the solid fissile limestone, and in thin layers of shale associated with it. The fossils are present in considerable numbers but so much scattered through the rock that large masses must be broken up to obtain a few organisms.

I have also found a few specimens of *Lingulepis* in the extension of the western fork of this hill in the second and third fields beyond, where it becomes a blue calcareous shale (E).

No fossils have yet appeared south of Mr. Smiley's property, that is, south of the fourth field from the driving-park, except some Stromatoporoid forms which are not infrequent throughout the Potsdam strata. From these localities I have already collected over five hundred fossils; some of these are in a perfect state of perservation, while many are in a very imperfect and fragmentary condition. There is no doubt whatever as to the presence among them of several well-known and well-marked Potsdam fossils, while the whole group appears to belong to that geological horizon. Since, in my

explorations among these limestones during seven years, all of the abundant fossils found have indicated the presence of either the Calciferous or of the Trenton formations alone, I have not failed to consider the question carefully whether the present fossils, though of a Potsdam type, may not after all exist here in Calciferous strata. But I have found no facts to favor the latter supposition. The rock is lithologically considerably different from any known fossil-bearing Calciferous in Dutchess county. The fossils are all of the type so well known in the Potsdam rocks of New York state and of Wisconsin, while no single Trenton or Calciferous fossil of the vicinity or of any locality, has appeared among the hundreds here collected.

These fossils will require a very careful study for a full determination as to the number of genera and species. The following preliminary statement will indicate their nature in general:

1. *Lingulepis pinniformis;* several specimens of both valves, some of which are quite perfect.
2. *Lingulepis minima;* many specimens of both valves.
3. *Lingulepis acuminata,* probably; several good specimens.
4. *Obolella* (*Lingulella*) *prima;* several specimens.
5. *Obolella;* a minute species resembling "*Nana*"; one or two specimens.
6. *Platyceras;* undescribed species; one or two.
7. Remains of small encrinal columns; several.
8. *Ptychoparia* (*Conocephalites*); n. sp. resembling *Iowensis*, but possessing an occipital spine; several glabellas and pygidia.
9. *Dicellocephalus;* two or more species; undetermined; numerous specimens.
10. *Ptychaspis;* one or more undetermined species: several specimens.
11. *Stromatocerium?* undetermined; abundant.

The more abundant of these fossils are the *L. minima*, whose fragments are thickly scattered through some portions of the shaly limestone, and next the movable cheeks of trilobites.

It will be observed, as has been suggested to me by Mr. R. P. Whitfield, that the fauna of this locality forms

a connecting link between the known fossils of the Appalachian region and those of the western states. Thus with the *Lingulepis minima* and *L. acuminata* of the New York Potsdam is found the *Lingulepis pinniformis* of Wisconsin and other western localities.

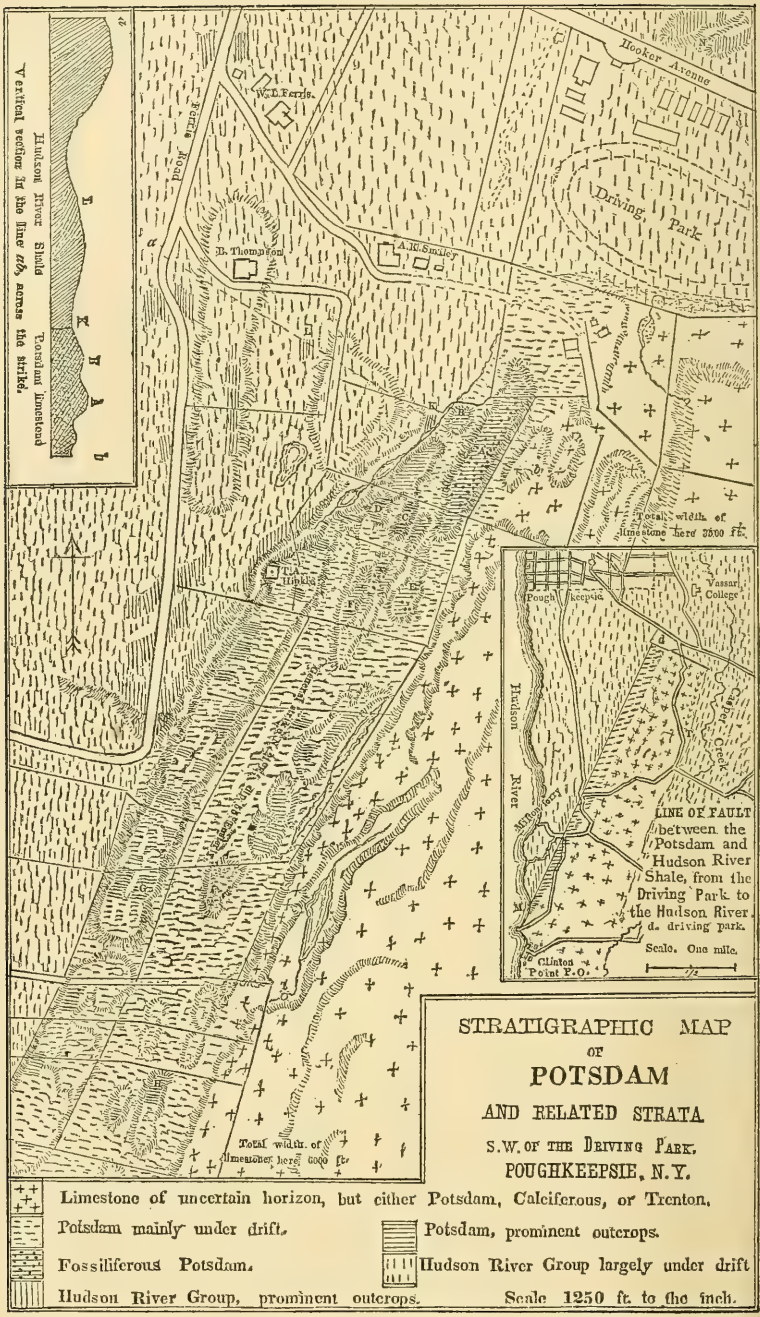
It may also be remarked that the fauna of the Poughkeepsie locality, as far as at present developed, is quite distinct from that of the Potsdam strata recently discovered by Mr. S. W. Ford at Schodack Landing and other places in the town of Stuyvesant, N. Y. From these localities, lying about fifty miles north of Poughkeepsie, Mr. Ford reports the following eleven species: *Paleophycus incipiens*, *Obolella crassa*, *Stenotheca rugosa*, *Hyolithes americanus*, *H. impar*, *Hyolithellus micans*, *Microdiscus lobatus*, *M. speciosus*, *Conocoryphe trilineata*, *Olenellus asaphoides*, and *Fordilla troyensis*.

No one of the above list has been found at Poughkeepsie; the distinctive character of the two faunæ is evident, the latter apparently representing an earlier stage of life.

The discovery of the presence of fossiliferous Primordial rocks among the Wappinger valley limestones while it adds an extremely interesting feature to the geology of the region, contributes another complication to the difficult task of mastering the stratigraphy of Dutchess county.

NOTE.— Since the above paper was written I have conducted these researches further, and have traced the continuation southerly of the fault between the Potsdam and Hudson river shale to the bank of the Hudson river. This line, which had been traced previously only into W. S. Johnston's farm, has been found to continue, well marked as before by ponds and gullies, in a straight line till it crosses the Albany post road at the corner of the New Hackensack road near school house No. 2. On the west side of the post road it passes straight across R. T. Gill's farm, then crossing the road leading to the Milton ferry, and striking between the two roads leading in a southwesterly direction to the river. It terminates finally in a high bluff on the river near Mallory's moulding-sand dock and about one mile north of Clinton point post office. Throughout this course the Primordial limestone and the shales show themselves frequently in close proximity in outcrops. In Gill's farm both formations are mostly covered by hills of drift, but on this farm, just north of the Milton ferry road, both the Potsdam limestone (here as calcareous shale) and the Hudson river shales crop out distinctly. The latter yield at this locality some very excellent

PLATE I.



(By error this plate is referred to as plate VI. on page 131.)

and characteristic fossils, as encrinal columns, *Leptæna sericea*, etc. The terminal bluff at the river is composed of *both* formations, the fault running along its summit, but near the northwestern edge. As the extreme southwestern point of the bluff is reached, the line of fault drops down its northwestern side, and the shales at last disappear, leaving the point in possession of the limestone. After careful examination I am satisfied that some, if not all, of the moulding sand is produced by the decomposition of the arenaceous Potsdam. Indeed, the process may now be seen going on in the layers of sandy limestone. It is very probable that the Potsdam may now be further traced across the Hudson river somewhere near Marlborough.

In plate I will be found a small map showing the entire extent of the fault above described.

It is proper to state here that I have strong reason to suspect the presence of a parallel belt of Potsdam limestone, more than a mile to the east of the present one, in the most eastern of the three belts of limestone. The particular locality which has furnished grounds for the above statement is on the summit of the ridge about half a mile southerly from the mansion on the MacPherson (late Boardman) place. The rock corresponds closely in its lithological characters with that of the Smiley locality. I have found here but a single fossil—about half of one valve of a brachiopod, which, as far as it goes, corresponds perfectly with *Lingulepis pinniformis*. I believe it to be this fossil, and that consequently the rock is Primordial, but cannot say that the evidence is absolutely conclusive.

EXPLANATION OF PLATE.

A, Hill of fossiliferous Potsdam on Smiley's farm.
B, Hill of Potsdam, partially conglomerate, on Smiley's farm.
D, C, Extensions of hill B. D is largely conglomerate.
E, Hill containing much fine-grained, blue, thinly fissile calcareous shale of the Potsdam group; a few specimens of *Lingulepis pinniformis* have been found here.
F, Calcareous quartzyte, Potsdam group.

The lane between the wall and fence just south of the hills E and F is the southern boundary of Mr. Smiley's farm; at present date no Potsdam fossils, except stromatocerium, have been found in this belt south of this line.

G, One of the best localities for inspecting the Potsdam limestone and calcareous shale composing this long hill. Stromatocerium is found here. From the summit of this hill a good view is presented of the wide plain of Hudson river shale which extends westward from its base.
H, A small quarry of arenaceous limestone apparently Potsdam, in the field belonging to W. S. Johnston.
I, A high ridge of light and dark-colored limestones, well exhibited at the spring here indicated; horizon of the rocks doubtful.
K, Outcrops of Hudson river shale at the surface of the ground within a few feet of the Potsdam hill B.
L, Small quarry of compact, gritty layers of the Hudson river group, dipping at a low angle.
O, Hilly outcrops of Hudson river shale in field north of the driving park.
M, (In small included map), moulding sand and dock.
T, (In small included map), outcrops of fossiliferous Trenton on the eastern margin of this limestone belt.

FEBRUARY 10, 1886—FORTY-FOURTH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

The following paper was read:

THE INTERPRETATION OF GENESIS.

(CONDENSED FROM THE ORIGINAL PAPER.)

BY CHARLES B. WARRING, Ph.D.

In Prof. Huxley's paper in the *Nineteenth Century* for December, 1885, on the "Interpreters of Genesis and Nature," he cries out against the varying interpretations which have been given to the story of creation, in the first chapter of Genesis.

In my limited way, I sympathize with Prof. Huxley in his desire for something that will not change as new facts come up. I wish that I had the gift of all knowledge so that my physical theories would accord with all new facts. Scientists have learned to make their theories as best they can, in view of all truth known to them, and it is thought no disparagement that they change as new facts come up.

An interpretation made to reconcile Genesis with science needs remodeling with every advance in knowledge, while one made on its own merits will stand forever, leaving scientists to change their theories to suit their own notions.

The following rules, if observed, will remove the reproach so often thrown upon this narrative:

1. Words should be taken invariably in the meaning determined by lexicon and grammar, and by use elsewhere.

2. Where the English Bible differs from the Hebrew, the latter is supreme.

3. Every sentence is to be taken to mean exactly what it says.

4. The order is to be unchanged.

5. The account is not to be condemned for the erroneous statements or explanations which have been made by its friends.

The friends of this account may object to such intense literalism, but certainly its opponents will not.

Prof. Huxley's criticisms are based upon the assumed existence in the Mosaic account, of a certain "central idea, the maintenance of which," he says, "is vital, and its refutation fatal," viz: "That the animal species which compose the water population, the air population, and the land population, respectively, originated during three distinct and successive periods of time." This "central idea" is, as every geologist knows, false, therefore, he says, the story is not of superhuman origin. But does the Mosaic account contain that "central idea"? Is it his "idea," or is it Prof. Huxley's, or rather, a tradition, which Prof. Huxley has accepted without examination?

The only fair way to decide this is by an examination of the story itself.

It says that at a certain time the waters swarmed with water creatures, among which were "whales" and fowl. Prof. Huxley's "central idea" needs to interpolate; "and there never was any life in the waters, nor any fowl, before that." The Mosaic statement is true; as to previous life, it is silent. Prof. Huxley ventures to fill the hiatus with what he thinks Moses would have said, if he had said anything about it.

I read farther on, that at a certain time the earth brought forth cattle, beasts, and creeping things. Prof. Huxley's "central idea" needs to interpolate; "and there were no land animals before that."

He places a physical falsehood in the mouth of Moses, and then says: "How unworthy of scientific notice; how utterly false; two statements in a few lines, which every geologist knows are not true. Hence it is plain that the story is a myth."

But Moses is not responsible for what any one may have said, or believed. By our rules we are to take his account as it stands. Nothing is plainer than that

neither the water, air, nor land population described in this account, refers to the origin of life, *i.e.*, to the first living creatures on our globe. What right then has any one to say that the author of this story intended to speak of the first water, or land population?

It will add to the interest of this discussion if we go back farther than the work of the fifth and sixth periods, and note what is said about vegetation.

I read of herbs yielding seed and fruit trees. Such a flora certainly did not belong to a horizon[1] where the highest types were algæ.

The writer names plants of the very latest order—that is all. But Prof. Huxley's "central idea" would add: "and there was no vegetation before that." Everybody knows that there was vegetation for millions of years before, hence Moses stands convicted of falsehood for what he does not say, but which Prof. Huxley thinks he would have said if he had said anything about it!

Now, what are the facts of our world's history, and how do these statements of Genesis fit in with them? That I may run no risk of wrongly stating the consensus of geologists, I quote from Prof. Huxley's *Lay Sermons*, No. X: "The following proposition is regarded by the mass of paleontologists and geologists, not only on the continent but in this country, as expressing some of the best established results of paleontology. Thus:—Animals and plants began their existence together, and then succeeded one another in such a manner, that totally distinct faunæ and floræ occupied the whole surface of the earth, one after the other, and during distinct epochs of time."

"A geological fauna or flora is the sum of all the species of animals or plants which occupied the whole surface of the globe, during one of the epochs."

1 "On the same horizon" is said of fossils and strata of one age. Imperial Dictionary.

There were many "epochs," perhaps fifty, so many at least are named by Dana in his *Manual of Geology* (pp. 142, 143). I note a few which are of peculiar importance either in themselves, or in relation to this account.

In the earliest epoch, are found traces of marine plants only, and the lowest forms of animal life. Geologists call this the Archæan Age.

In another, the lower Silurian, many centuries later, sea-weeds and protozoa have added to them, radiates, mollusks, and articulates.

In another, perhaps thousands of centuries afterwards, all these, plus some land plants, are found, but no fruit trees. This is the upper Silurean.

In another, centuries later, more land plants, but no fruit trees, and in the waters, fishes. The Devonian Age.

In yet another, there were the same types of animal and vegetable life, plus land animals, but still no trees whose fruit enclosed the seed; there were land animals, but no mammals. The Carboniferous Age.

In a yet later epoch, the Cretaceous, there is found added to a varied animal life, and to old types of plants, a flora which includes herbs yielding seed and trees bearing fruit whose seed is inside of it, but not of present species.

Later again, we come to an epoch, the Tertiary, that contains grasses, herbs yielding seed, and fruit trees bearing fruit. Dr. Newberry, in his address before the Torrey botanical club, published in their *Bulletin* for July, 1880 (page 79,) says: "Our present flora is but a relic of that of the Tertiary. We have already collected from Tertiary strata in various parts of our country the remains of more species of fruit trees than are now growing on its surface." The flora of the Pliocene—the last of the Tertiary—is largely yet living.

As to the animals of the Tertiary, the fishes, whales, birds and mammals, Prof. Dana, page 518 of his *Manual*, says: "All the fishes, birds, reptiles and mammals are extinct species."

After this there were several—not many—successive epochs. One in the Quaternary, contained a fauna of fishes, amphibians, or reptiles, mammals and birds. There were whales and seals. Most of the birds are still represented. Some however have died out very recently; say within a century or less. Of the others, save the mammals, all kinds, so far as known, are still in existence. The mammals are nearly all extinct.[1] Whether the whales are all extinct, I cannot say. Perhaps "*Tannim*" should be rendered great sea monsters, or simply great fishes; in any case they were great water vertebrates.

Coming still farther down the record, we arrive at the last epoch, that which is characterized by the presence of living species of cattle, beasts and creeping things.

In brief, then, we find many successives epochs, none of which contain the plants, or animals, named in Genesis, but near the end of the long series, viz, in the Pliocene, Quaternary, and Present, we find in the first, a flora with grasses, herbs and fruit trees as of today, in the second, "a water, and an air population, and (in the third,) a land population" identical with those now living.

Putting the two records side by side, we may more easily compare them.

GENESIS.	GEOLOGY.
Grasses, herbs and fruit trees are placed before the "living" creatures of water, air and land.	Grasses, herbs and fruit trees appeared before living species of water, air and land animals.[2]

1 Page 335, Nicholson's Ancient Life History. Also Dana's Manual Geology, page 563, 3d ed.

2 Dana, Man. Geol. page 515.

GENESIS.	GEOLOGY.
After such a flora, we are told that God created great whales and living water creatures and fowl.	After fruit trees and the other plants, whales and other water creatures of living kinds, and fowl, of present species, made their appearance.
After all the above, we read: Let the earth bring forth the living creature after its kind, cattle and creeping thing and the beast of the earth. And it was so.	After living plants, and present water animal, and fowl, cattle and other living land animals, and beasts made their appearance.

Here, so far as I can see, is perfect agreement in the statements and in their order.

As to all other matters pertaining to life, its existence prior to these last three epochs, its mode of development, and many other things, about which "science" gives us facts, or theories, it is silent, but silence is not falsehood.

I have purposely said nothing about man, or to what horizon he belongs. I have but little to say now. Until geologists know much more than they do, it will be impossible to speak with certainty of the date of his appearance.

Our inflexible canon is to be applied here also.

"The account is responsible only for what it says."

As I read the record, God made cotemporaneously with, or subsequently to the cattle and beasts, a pair of human beings whom he named Adam and Eve. I am barred by this literal view from adding, "and God never made any other men." I do not know whether he did or not. Indeed there are intimations in other parts of Genesis, that are hard to understand on any theory other than that a prior race existed. I refer to Cain's fear that whoever met him would kill him, and to the sons of God marrying the daughters of men.

I do not know what the truth is. The lesson taught by the failures of others, should teach us to wait in patience, believing that the truth can take care of itself.

To this literal rendering of the account it may be objected that it runs counter to the belief of the world, for it has been held that Moses recorded the introduction of life, and not the close of the long series of "horizons" which preceded the present.

But to scientists, such as Prof. Huxley, and those of similar belief, this will have little weight, since, being embarrassed with no theological bias, the question with them will be—or at least ought to be—Does the story say what has been attributed to it? and is Prof. Huxley's "central idea" an interpolation for which the author of this account is in no wise responsible?

That it is an interpolation follows from the spirit of the rules laid down by us. "The account is responsible only for what is in it." To make the injustice of Prof. Huxley's claim more evident I add his "central idea" to each statement. It then becomes glaring.

And God said, Let the earth bring forth grass, the herb yielding seed, and the tree bearing fruit whose seed is in itself, and it was so. *And this was the first vegetable life on the earth.*

And God said, Let the waters bring forth abundantly the moving creature that hath life, and fowl that may fly in the expanse of heaven. *And this was the beginning of animal life on the earth. No water, air, or land animals had lived till then.*

And God said, Let the earth bring forth the living creature after his kind, cattle, creeping things and beasts of the earth. And it was so. *And these were the first land animals in the whole earth.*

Very small geological knowledge will suffice to show that the falsehood, that "by which the account must fall," is wholly in the added, "central idea."

If not that, what, then, is the central idea? So far as I can see, it is simply God's creatorship of all things. It shows itself everywhere, and markedly in those cases

where the fiat is followed by a statement broader than the fiat itself. After calling for certain creatures with which the water was to swarm, it tells us in the next verse, that God was the creator of them and of "everything that moveth."

And so in regard to land animals; the fiat calls for certain creatures. The next verse says, God was the maker of them, and of every "creeping" (or moving) thing. Hence the story claims for God creatorship not merely of the fauna which came into being then, but of any and all, even though their pedigree ran far back into the past. There were in each of these horizons animals coming from horizons farther back, some reaching (notably various mollusks) from the Eocene to the present day. God's creatorship is made to include them also.

It is evident such a central idea might have been set forth in various ways. The writer might have made a bare statement that God made the universe and its contents. Or to have impressed it more deeply, he might have gone into details, and, taking item by item, in any order that they met his eye as he looked about him, have said that God made each. Thus he might have said:

God made man.

God made the cattle and all land creatures.

God made all water creatures.

God made all plants.

God made the sun and moon and stars.

God made the dry land and the sea, and so on.

It all would have been true, and it would have been a mental flight far beyond anything else in ancient literature.

It is possible, too, that the writer might have placed the plants and animals in their true order. One might guess right two or three times, but when we consider that this harmony, or order, is even more marked in the

earlier periods, and that on a careful analysis this story touches our most recent science in more than thirty important points, and that each statement is in its proper order, the human mind refuses to attribute the coincidences to anything less than absolute knowledge on the part of the author of the account.

One other thought and I am done. It was science that needed to be reconciled with Genesis. For thousands of years, it was far enough away, but at last, unwittingly it is true, it is a witness to the divine origin of this account.

FEBRUARY 24, 1886—FORTY-FIFTH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

The following paper was read :

THE TOP.

A PAPER SUPPLEMENTARY TO ONE READ BEFORE THE SECTION IN 1885 ON GYRATING BODIES.

BY CHARLES B. WARRING, Ph.D.

In my paper on gyrating bodies last winter, I said that a top when revolving on a hard, smooth surface, would rise from an oblique to a vertical position, if its "point" was of sensible size, or if it, the point, was "out of centre."

This experience is so common that no dispute as to the fact is possible, the only question is as to the reason why. In my paper I offered what seemed to me the true explanation, and summed up as follows : The rising of a top is due indirectly to its traveling, or perhaps it would be better to say : If the "point" cannot travel, *i. e.*, change its position, the top cannot rise to a vertical. Mr. Blish, of Niles, Mich., writes that this cannot be true, for he caused a top with a roundish "point" to revolve in a small, cup-like depression, and

that nevertheless it rose to a vertical. I had observed the same thing, but did not mention it in my paper, because in all my experiments where this occurred, the depression had been somewhat larger than the "point" and consequently permitted it to travel around in a small circle, hence I could not see that there was any real contradiction of what I had said.

After receiving Mr. Blish's letter I devised what seems to me an *experimentum crucis*, for it permits freedom of rotation and of gyration,[1] and yet prevents the "point" from traveling. The arrangement for this purpose is shown in figure 1, where is presented a vertical section

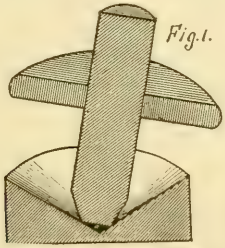


Fig. 1.

of a hollow cone in which the circumference of the "point" touches all around, thus preventing any motion of translation. The "point," it will be noticed, is of considerable size, and flat — a form most favorable for causing a top to rise. On trial I found that the top, although revolving freely, both on its own axis and about a vertical one, would not rise. I then placed it on a pane of glass, thus allowing it to travel, and it rose quickly to a perpendicular.

I think this suffices to show that Mr. Blish's experiment proves only that the "point" of the top does not need to travel very much, and as in that I agree with him, I do not see that he proves anything against my statement.

1 All through these papers, gyrate and gyration are used to denote the revolution of the gyrating body around a vertical axis, while rotate and rotation refer to its revolution on its own axis.

Last winter I spoke of the curious tracings made on smoked glass by the point of a top when left to take its own course. I did little more than speak of their exquisite beauty, and exhibit some specimens by the photoengraving process as shown on plate IV, vol. III., of our *Transactions*. Although, unfortunately, the tracings are there reversed, and consequently not well adapted to exhibit the law which governs these movements, still they serve a useful purpose in some respects, and hence the plate is reproduced as plate II., in this paper. It represents parts of three tracings. The largest, or number 1, shows only one and a half gyrations, when for some reason, I forget what, the top was lifted from the glass plate. The axis was much inclined, and the point (about one-fortieth of an inch in diameter) a little "out of true," hence the wavy, broken appearance. Number 2 was made with the same point, and with axis less inclined, the waviness continued, but was more pronounced. This was due to the axis being more nearly vertical. The "point" jumped clear of the coating of lamp-black in case of number 1, but pushed it back in case of number 2.

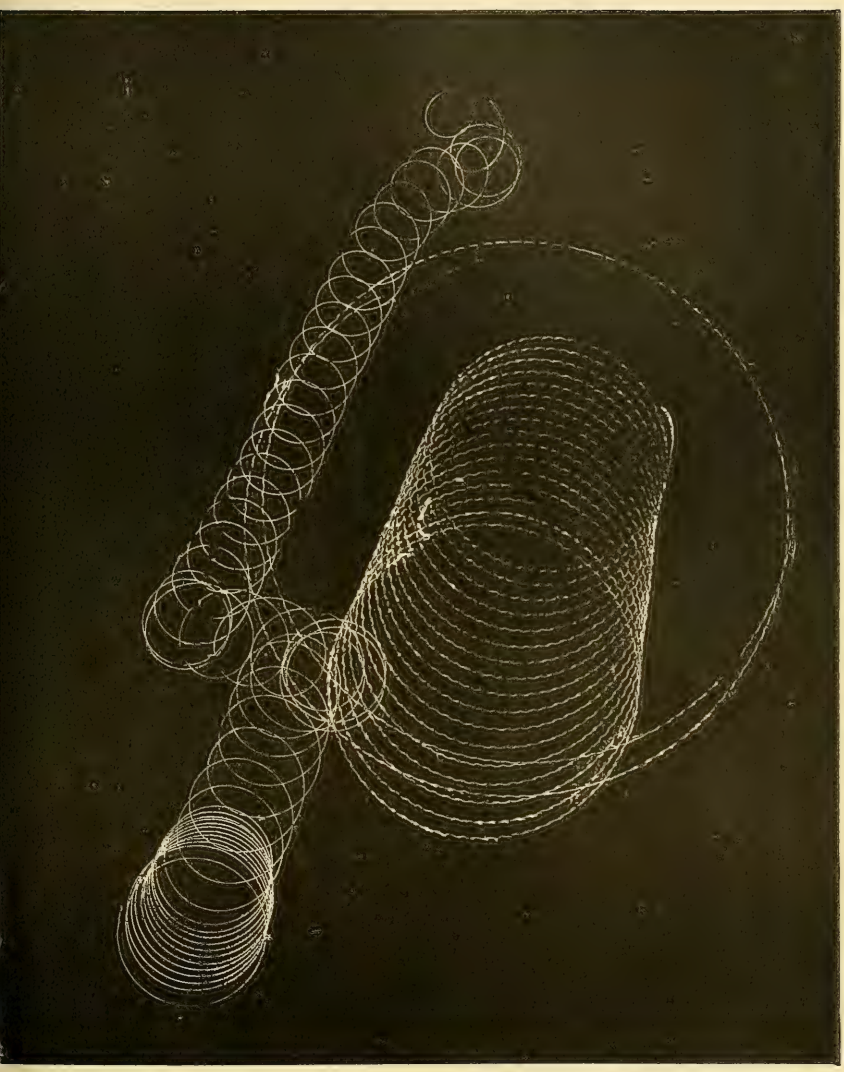
Number 3 was made with a needle point, better centered. It is noticeable for its freedom from waviness. The axis, I think, was inclined about as in number 2.

The following laws can be experimentally proved :

1. The first spiral is the largest ; the others grow smaller with great uniformity till near the end of the top's running, when they begin to grow larger, but with far more rapid increase of size. In a very few instances I have seen the spiral commence small and increase to the end.

2. On a pefectly level, and uniformly smooth surface the top does not travel. The point simply traces a series of concentric spirals.

PLATE II.



3. If the surface is inclined, the top crosses it at right angles to the inclination, and travels towards that side of the spiral which is up hill.

4. To change its direction, change the inclination at right angles to the way you wish it to go.

WHY THE FIRST CURVE OF THE SPIRAL IS THE LARGEST.

Let figure 2 represent a top exaggerated for convenience of representation, the actual "point" being less than one-twentieth of an inch across. If much larger than this, the top will rise so quickly, and describe a path with so large a radius, that it is difficult to study it. The face of the "point," except where the contrary is stated, is flat.

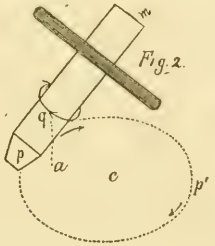


Fig. 2.

Suppose the top at the moment represented in our diagram to be revolving rapidly on its axis in the direction indicated by the ellipse around the shaft. The upper end, m, will be approaching the observer, and the lower end, p, will be receding from him. In other words, the instrument is gyrating around the instantaneous axis, q a, both motions being with the hands of a watch.

The rolling of the "point" would carry the top in a straight line from the observer, if its axis kept parallel to itself. But as m swings around the vertical q a, p correspondingly changes its direction, and the distance which it (p) will travel while m is swinging through,

e. g., 90°, will depend upon the size of the "point," the rate, n, at which the top revolves on its axis, and the slowness of m's movement, *i*, *e.*, the slowness of the gyration.

Hence, since the size of the point is constant, and n is greatest and the gyration slowest, at first, it follows that the first spiral will be the largest. As n grows less, the gyration becomes more rapid, hence the next curve will be smaller, the next, smaller yet, and so on.

As the gyration, the inclination, and the rate, n, may vary indefinitely among themselves, the rate of diminution in the diameter of the spirals will vary almost indefinitely.

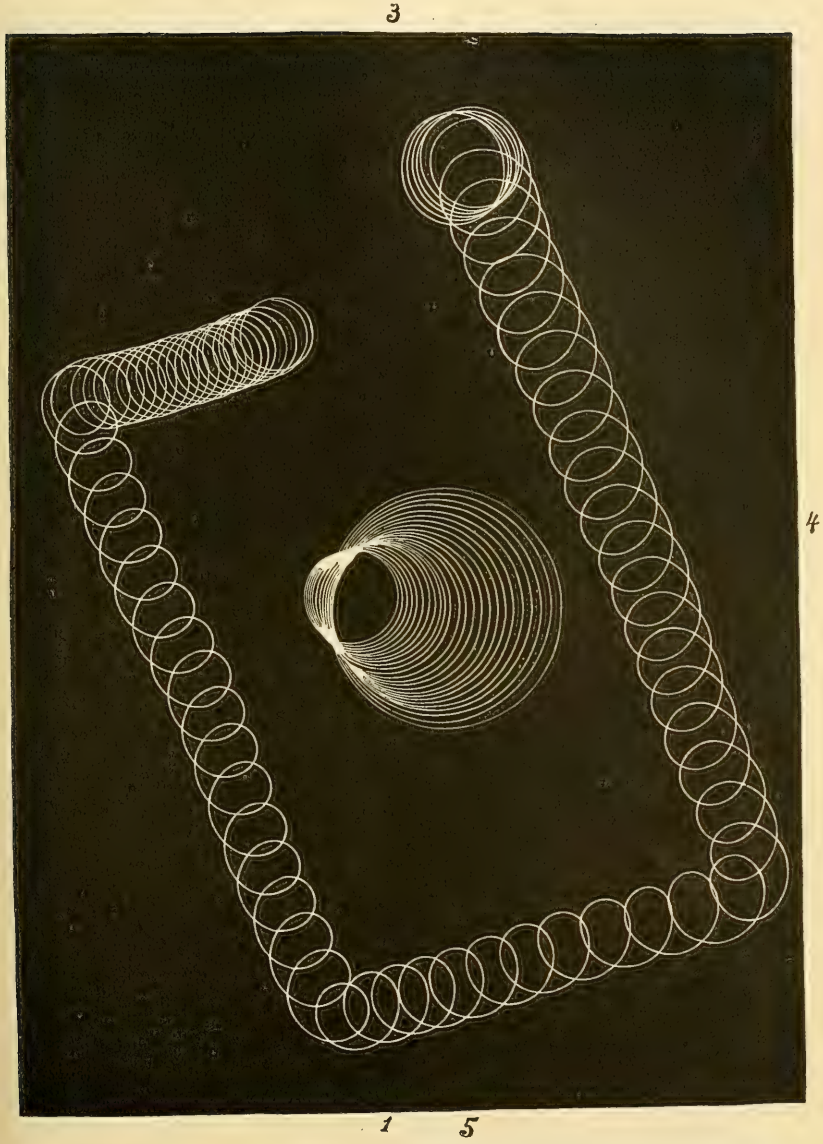
THE TRAVELING OF THE TOP.

It would seem from what has been said that the tracing should be a concentric spiral, commencing at the outside. It will be found on trial exceedingly difficult to get the top to make such a path. To approximate to it, the surface must be a plane as nearly level as it can be made. The central figure in plate III represents a tracing on polished plate glass, laid as horizontal as possible with the aid of an ordinary spirit level, yet there was a movement to the left. I have often had tracings in which, although the centre of the spiral travels a little, all the curves were within the larger one, but never one where they were absolutely concentric.

The other tracings on this, and on plate II, show abundant movement. These illustrate the third law. We will try to show their rationale. Let figure 3 (page 155) represent a bird's-eye view of our top, inclined as in figure 2, and in four successive positions, the pin, or shaft, alone being represented.

The body of the top may, for the moment, be considered as so transparent as to be invisible. The arrows show

PLATE III.



The small figures indicate the order in which the sides were elevated to form the rectangular path.

the various movements, all easily deduced from the rotation on the axis.

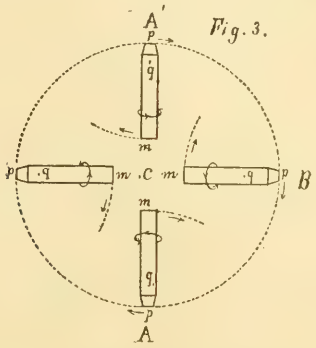


Fig. 3.

Suppose that the top at the instant we begin to observe it, is passing A. As m swings slowly to the right, the point's path begins to curve to the left, and will continue to curve just as m does, both completing 180° in the same time. If m's motion around the instantaneous axis q, is uniform, the curvature of p's path will also be uniform, and the path, a circle. If p revolves on its axis very rapidly, it will travel forward very rapidly; and since m's rate of curvature (its gyrational movement) is proportionally slow, the rate of curvature of p's path will be correspondingly slow; and the slower the curvature the greater the distance p will travel (roll along) to get through a quadrant. In other words, the radius of its path will vary directly as n, the number of revolutions which the top makes on its axis per second.

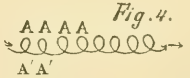
If by any means the forward movement of p is diminished, while the angular, or gyrational, movement of m remains unchanged, the radius of the path will grow smaller, since p will travel a less number of inches while m is swinging around 180°; and, conversely, if the translational speed is increased, p will travel more inches

while m is swinging through 180°, or in other words, the radius will be larger.

Now we will apply this principle. Suppose our glass plate to be inclined a small amount to the horizon, the side toward us being the highest. Then as p passes on from A, the action of gravity will accelerate the movement on the left hand half, and retard it on the other. The velocity due to gravity will be zero at A, greatest at A′, then diminishing back to zero at A again.

The curvature of the path, since the rate of gyration is not affected, should diminish from A to A′, and increase from A′ to A. Hence the path should be flattest at A′, and round most rapidly at A, something like figure 4.[1]

Fig. 4.



So strongly marked a loop form will occur only when the plate is considerably inclined. Such may be seen in plate III.

Further consideration of these movements will show that as p ascends towards A, the curvature increasing as has been shown, the radius must become shorter in the same ratio, hence the point will not rise as high as the first A, hence the loops will grow smaller as the series extends. Hence, too, the instrument will gradually work its way towards the side on which the radii are growing less. In the case before us where A is the highest (fig. 3), it will move towards the right, and its rate of movement will depend upon the inclination of the plane, the former growing more rapid as the latter increases.

By availing oneself of this principle, it is easy to guide the top and send it to the right or left, or back and forth.

1 This is copied from an actual tracing by my top, but does not adequately represent the exquisite regularity of the path.

But as the motion is always different from what one unacquainted with the peculiarities of gyrating bodies would expect, and is very apt to confuse him, I have found it very convenient to use a direction diagram, like figure 5, which I place plainly in view when experiment-

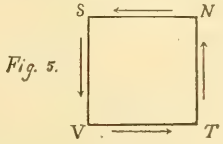


Fig. 5.

ing. If I tilt up the side of the glass plate corresponding to V T, the top goes to the right ; if I tilt up T N, it goes from me, and so on ; in each case the arrow denotes the direction when that side is highest. It must be remembered that here, and all through this paper, the top revolves with the hands of a watch, otherwise the effect would be reversed. Plates II and III show paths so produced and guided. It will be noticed, however, that the path is rarely parallel to the sides of the plate, but slopes away from it. This is due to the steady downward pull of gravity, causing a certain amount of slipping, or perhaps, it would be better to say that the oblique path is the resultant of two forces, the pull of gravity tending to send the instrument directly down the plane, and the lateral movement, which tends to send it, as a geologist might say, along the strike.

It will readily be seen that if the glass plate is rectangular, the corresponding changes of direction must also be rectangular as in all the paths shown in the engravings. If the glass plate is, say, an equilateral triangle, the resulting paths will be a similar triangle. To do this satisfactorily, the experimenter will need a guide as for the previous paths. Draw a small triangle on paper, so that the base is towards the observer. Then at each vertex draw a short arrow parallel to the side opposite, so that the heads point in the direction

opposite to the motion of the hands of a watch. Call the right hand arrow A, the upper one B, and the left hand C. Raising A makes the top travel from the observer and parallel to the side opposite A, and so on.

By using glass plates of various forms, a variety of figures can be produced of great beauty.

I have said that the radius of the curve on the up-hill side is shorter than that on the down-hill side, or in other words, the radius is shorter on the side towards the end of the path. That such a difference will account for the traveling of the instrument and for the space between the curves on the side from which it is going being greater than on the other side, can be proved experimentally by a process the result of which

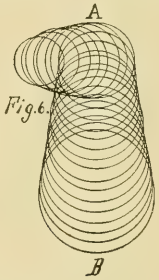


Fig. 6.

is shown in figure 6, described by a compass-pen. I merely shortened the radius for each of the curves towards A (commencing at the largest end, B). By changing the shortening operation from the front to one side, I caused the spirals to change their direction.

The less the inclination of the plate the less the distance between the spirals. They may thus be made to over-lap and almost coincide.

This is very prettily shown in plate II. In number 2 (numbering the three by size) the plate was not touched during the tracing, consequently the distance from curve to curve is exceedingly uniform in its progress from first to last. But in number 3 the rate changes abruptly. For

the first two curves, the glass was considerably inclined, something being accidentally under one side. This was drawn out as soon as seen, and the plate became almost level, the same side however being yet a very little the highest; at once the curves closed up. After about ten curves had been traced, I raised that side higher than at first; the effect is seen in the greater distance between the subsequent spirals. As this plate was unfortunately reversed in photographing it would lead to confusion were I to trace the path through its changes of direction.

In plate II this was avoided, the lines here being in the same position as in the original. The "point" used was only about one-fortieth of an inch across its face, and the axis inclined 25° or 30° from the vertical. The smoked glass plate was as nearly level as I could make it. The top began its course at the larger end of the spiral, the plate standing before me as it does in the engraving. The surface, however, was not level; the movement of the spirals towards the right and their closeness together showed that the end nearest me, was slightly elevated. (Turn to fig. 5 and note the arrows). As soon as I could, I raised the left hand side about one-half inch (about 8° to 10°). The result was a movement towards me, which, with the down-hill pull, resulted in the oblique path seen in the engraving. I then raised the side furthest from me about the same amount. Then the right hand side, and last, but not so high, the side next to me.

With the help of the key afforded by figure 5, it is easy to trace the course of the most complicated paths, or to map out a course in advance.

MARCH 24, 1886—FORTY-SIXTH REGULAR MEETING.

Charles B. Warring, Ph. D., chairman, presiding.

The evening was occupied in general conversation on scientific subjects.

APRIL 13, 1886—FORTY-SEVENTH REGULAR MEETING.

Charles B. Warring, Ph. D., chairman, presiding ; fifteen members and fifty guests present.

The following paper was read :

THE NICARAGUA CANAL.

BY CAPT. HENRY C. TAYLOR, U. S. N.

Mr. Chairman, Ladies and Gentlemen : In speaking to you to-night concerning the problem of connecting the oceans, it may be well to say in advance that I am a firm believer in a canal by way of lake Nicaragua. I will add, however, that I have been brought to that belief by no interest in one route over another, but by unprejudiced study of all the routes for many years, and by some personal observation and experience.

We will consider principally three localities and methods by which the oceans may be joined : Panama, a sea-level canal ; Nicaragua, a canal with locks ; Tehuantepec, a ship railway.

Upon these three routes the interest of the world's commerce has centred. There have been others — some of them with ardent advocates,— and a slight notice of them may do something to clear the ground before we come to a more detailed consideration of these three principal lines.

The search for a practical canal route succeeded to the long, persistent examination which had caused explorers for so many years to penetrate every inlet from New-

NOTE—In preparing the following paper I have made free use of the excellent work of Lieut. Sullivan, U. S. Navy, from which I have obtained much valuable information. I wish, also, to acknowledge indebtedness to Mr. Rodriguez' late work of interest on the Panama canal ; to pamphlets kindly sent me by Capt. Eads and Mr. Corthell of the proposed Tehuantepec railway ; to officials of the Panama railroad, of the Panama canal, and of the state of Panama in examining the work on that canal ; and officials of the republics of Nicaragua and Costa Rica during my brief visits to those countries. My thanks are above all due to Admiral Ammen and Engineer Menocal, U. S. Navy, to whose great knowledge and ability and patriotic disinterestedness the world will some day be largely indebted for the possession of a Nicaragua canal.

foundland to La Plata, in the hope of finding the strait which they confidently believed nature had provided as a means of communication between the oceans.

"Men," said Humboldt, "could not accustom themselves to the idea that the continent extended uninterruptedly from such high northern to such high southern latitudes." From the year (1513) when Nuñez de Balboa first looked upon the wide sweep of the Pacific, a century was occupied in fruitless efforts of gallant and capable men to discover that strait which nature should have placed there — but did not.

The Cabots worked in the north. D'Avila, under secret orders of the Spanish king, scrutinized eagerly the isthmuses and the Spanish main; while DeSolis, under similar instructions, explored the coast of Brazil, and while hopefully ascending the great estuary of La Plata, was killed by the natives of that region. Ponce de Leon sailed hundreds of miles northward from Panama on the same errand; Cabrillo and other lieutenants of Cortez groped north and west from Tehuantepec, as far as the vicinity of the present Monterey and San Francisco; and Cortez himself, under the urging of his royal master, King Charles V., of Spain, struggled against much obstacle and disaster to achieve the desired discovery. When, however, the gulf of California was found to have a head at the mouth of a great continental river, the intelligent Spanish explorers, already doubtful, could no longer believe in the existence of any communication between the seas, and the "secret of the strait" faded away into the dreamland of legend and fable. Other nations than Spain still hoped. As late as 1607, we are told by Bancroft, Virginia colonists were ordered to seek communication with the South sea "by ascending some stream which flowed from the northwest," and that it was in ascending the Chickahominy with this end in view, that Captain John Smith was cap-

tured by the natives. And thus another touch of interest is added to the adventurous record of this man of ordinary name and extraordinary life.

The strait was indeed an idea difficult to surrender. It ought to be true, they said. The seas are so close together for a thousand miles. Commerce between "Cadiz and Cathay" so greatly needs it. It must be so. That it should not be, was, in the words of the writers of that day, "repugnant to the interest of humanity." The "secret of the strait" must be disclosed.

Not alone in history are these ardent enthusiasts. In every age some will be found. In our own day we have seen two men of brilliant parts gazing upon the map of Central America with the eyes of conquerors. The one saying "I perceive how useful to commerce would be a canal at Panama. Therefore nature must have intended one. Therefore, nature did intend one and I will dig one. And now, that being decided I will enquire as to how much digging there may have to be done," what mountains to remove, what torrents to control. Another, also with map on knee, says: "I perceive the commerce of the Mississippi, issuing into the gulf of Mexico, opposite the isthmus of Tehuantepec. How natural and simple to traverse the gulf, and crossing the isthmus, to proceed to our Pacific states. Nature clearly contemplated this location for a transit route. I will utilize it. And now, with that settled, let us see what obstacles to remove, what serious difficulties to surmount. Let us see whether in fact nature did contemplate this location as a transit route." As in the former time, these gentlemen know what nature ought to have done, and therefore know she has done it. How much this false knowledge of the one has cost the people of France can now be approximately estimated. We have yet to learn at what cost the people of the United States will acquire the same valuable though bitter experience.

The world moving, like all large bodies, slowly toward conviction, did become at last convinced that nature had not pierced the barrier for our use and comfort, and this conviction once forced upon it, plans for an artificial channel began soon to be suggested. The idea had been touched upon by Balboa, Cortez and Saavedra, but the first record we have of a practical suggestion is that of the Spaniard Gomara, who urged the idea upon Philip II. in 1551,—but the son was not the father; nor were such leaders as Cortez to be found, even had the spirit of Charles V. still animated the actions of the Spanish throne.

From this time forward, the Spanish government seemed disposed rather to smother than to encourage any efforts to connect the oceans. As the old-time Spanish vigor departed, the feeling grew that if any good route were found, it would only be snatched from them by some of those daring Drakes and Grenvilles, who, roaming the seas at the head of brave and reckless companies, sought every opportunity to insult Spain and plunder its colonies.

A long period now passed, during which no interest was evinced in the canal question. The mystery with which the Spanish government had wished to cover it was complete. If the desire for knowledge came later, the failing vigor of that nation stood in the way of any successful investigation.

It was left for Humboldt to reawaken an interest among the nations, and to indicate localities where favorable results would be most likely to be met with by the explorer and surveyor. In his opinion, the valley of the Atrato and the isthmus of Darien were points where examinations should first be made. Later we shall see how thoroughly these localities have been surveyed, and with how little success.

In 1825 Nicaragua invited the co-operation of the

United States in the construction of a canal by way of lake Nicaragua and the river San Juan, but with no satisfactory results. In the same year Mexico caused a rough survey to be made of the line *via* the Coatacoalcos river and the isthmus of Tehuantepec, resulting in an official report that the "canalization of this isthmus was problematical and gigantic."

Later, in 1828–29, a survey was made under orders from General Bolivar of a line substantially the same as that of the present railway between Aspinwall and Panama. Nothing was effected by this action, except to put an end to the popular error that the mean levels of the oceans on opposite sides of the isthmus differed appreciably. There are still some, I believe, among those who have not given their attention to this subject, who are yet ignorant that differences are caused only by tides, winds, barometic pressures, and other temporary disturbing causes, and that the mean levels of the two seas may be regarded as practically the same.

Many other attempts were made in the ensuing years, and with uniform lack of success. In 1830 the Netherlands, beginning fairly enough, were obliged by the revolution in Belgium and its separation from Holland to give up the project. In 1835 President Jackson appointed Mr. Charles Biddle as special agent to promote the idea of an isthmus canal, and to visit the Central American countries for that purpose. Mr. Biddle met with many difficulties, and returned to the United States with nothing accomplished of value, and from this time the projects became too numerous to be even touched upon in a paper of this scope. M. Guizot, under Louis Philippe, urges interoceanic canal questions upon the attention of the Chambers of Deputies. A bishop of San Salvador goes to Rome and urges the importance of a canal upon the Pope. All of no avail; but in 1849 a success is scored, not for a canal, but for the Panama rail-

way ; a means of transit of vast service to commerce, but which, by providing an imperfect method has perhaps retarded the realization of the greater demand for a water communication between the oceans.

We must now, having glanced briefly at the past history of this great problem, make a hasty review of the work of to-day, and consider the various routes which have been examined duing the last few years. Although the boiling-down process of precise instrumental surveys has reduced the possible lines of transit to three, Panama, Nicaragua and Tehuantepec, and although the further boiling-down process of actual digging and building will, it is believed, soon rule out Panama and Tehuantepec, leaving only Nicaragua, yet many other routes, methods and plans have been examined, and no portion of the isthmuses can be said to have been neglected.

Beginning at the south, we find the Atrato river, recommended by the great Humboldt, rising in the mountains of Western Columbia, and pursuing a northerly course to its mouth in the southwestern corner of the Caribbean. Although its waters empty into the Eastern sea, its course is parallel to the Pacific coast, and only about fifty miles from that ocean. The main stem of the Andes, whose eastern slopes it drains, separates it throughout its course from the Pacific. This range is throughout this portion not of great elevation, and numerous tributaries afford, in their valleys, easy grades from the Atrato to or toward the crest of the divide.

Humboldt was told of vessels passing from the headwaters of the Atrato to those of a small stream flowing southwest into the Pacific,—by means of a short canal. Later examinations show that the vessels were canoes. the canal, if not mythical, was a ditch, and that a long and high portage intervened over which the canoes

were dragged. Lower down the Atrato several lines of levels were run across the divide with much care and labor, following the lines of some of the principal tributary streams entering from the westward. In these it was found necessary that the summit levels should include tunnels many miles in extent, high enough to accommodate ships with at least their lower masts left standing, and involving enormous expense. Attempts were also made to connect the gulf of San Miguel with Caledonia bay on the Caribbean side: and at a point farther west, to connect the gulf of San Blas on the Caribbean with the Bayano river, emptying into the head of Panama bay. Here again long tunnels or other formidable obstacles were soon revealed as the lines of levels were carried across.

Next came the line of the Panama railroad, and here high hopes were entertained, for a railroad was already there, and the Chagres, a large stream, debouching near Aspinwall, has its source well over toward the Panama or Pacific side of the isthmus. After careful surveys on this line it was decided that a lock canal was possible, though difficult, costing over a hundred millions, and meeting with some trouble in supplying water for its summit level. A canal at the level of the sea was deemed impracticable, it being considered that the violence of the freshets in the Chagres placed it beyond successful engineering control.

The surveys of these routes had been carried on by our government, but the interest felt to-day in the Panama canal project makes it proper for me to notice other work in that locality, for French enterprise had begun to stir, and a speculative company, known as the "International Society of the Interoceanic Canal," was formed in Paris in 1876, It sent out an officer of the French navy, with instructions to search for the best line for a canal within certain boundaries named in his

orders. These boundaries were the limits of certain territory in which the society had reason to know they could obtain a concession of land for canal purposes. From the sale of this concession the society hoped for much profit, but was not specially interested in the final success of a great canal company, if first that company should have purchased from their society, at a sufficiently large price, the desired concession. This was not a good beginning, but the instructions to this expedition were submitted to M. de Lesseps, and approved by him.

The individuals comprising this expedition proved to be, in all save engineering qualities, vigorous and enterprising persons. They ran some lines of levels, glanced at some other routes, guessed at the heights of many hills, and at the possible volumes of many streams in time of freshet. On the basis of that data, however, they did not hesitate to make the most elaborate plans and estimates, including minute details.

The real and valuable work done, valuable to the speculators, at least, was shown, however, when they returned from France in 1878, bearing with them a concession, obtained at the capital Bogota, and embracing all routes comprised within the limits of the United States of Columbia. It seemed to matter little to the gentlemen of Paris that no practicable line existed throughout their concession ; that their most able engineer, M. Celler, in despair of finding any route for a sea-level canal, had submitted plans for a canal with locks, while admitting that he had but meagre data for it, because they had been sent out to find only a sea-level canal. These mischances affected but little the speculative minds of the international interoceanic company. They had obtained the concession ; they had, by means known to themselves, persuaded that illustrious Frenchman, whose fame gained at Suez made

his name a symbol and surety of success, to cast in his fortunes with them. There was left them only to use that name to form a great canal construction company, which should purchase from them, at a great price, the concession they had obtained from Columbia for a song.

To do this no time was lost. An international conference was held at Paris, under the auspices of the Paris geographical society, in May, 1879, for the purpose of deciding upon the best locality for an interoceanic canal.

With seventy-four French members devoted to de Lesseps' interests and ideas, and but sixty-two of other nations; with a "technical" committee and sub-committees crowded with Suez canal engineers, with a programme specially arranged to prevent general discussion,—with these precautions it is not surprising that, amid the sturdy protests of such world-famous engineers as Sir John Hawkshaw, of such special experts as Admiral Ammen and Engineer Menocal, the conference should vote with enthusiasm for a sea-level canal between Aspinwall and Panama.

The conference ended, a great company was soon formed; and de Lesseps at its head, by his reputation and his marvelous energy, soon had the needed millions at his disposal, and began the work which we are now watching to-day,

As to its progress: The expenditures are represented by something over one hundred fifty millions of dollars; while M. de Lesseps claims from twelve to fourteen per cent. of the excavation completed, and unprejudiced engineers claim only six to eight per cent. completed. He holds that the time already occupied, five years, has been so well spent in preparation, that three years more will complete the work. Neutral parties of intelligence announce that it will be impossible to complete it before the year 1900, even under most favorable circumstances. But the circumstances cannot be favora-

ble. The charges of interest upon money already spent will be an unceasing drain upon money yet to be received. Torrential rains must continue to fall during the rainy seasons. The problem of the unruly Chagres remains yet unsolved.

We cannot doubt the brilliancy of de Lesseps' vigorous intellect. His long career vouches for it. But Napoleon was brilliant, and yet committed the foolishness of invading Russia. He was great, but he had his Waterloo. De Lesseps is great, but he has his Panama.

Let us pass now to the north and west, to a locality where nature seems to have made, if not a perfect site, at least a disposition of land and water more favorable than at any other point, for a water transit between the oceans. Here the backbone of the continents and isthmus, running parallel and close to the Pacific shore, sinks to its lowest point, while its eastern slope descends into that great sheet of inland sea known as lake Nicaragua. At this low point the divide is less than fifty feet above the level of the lake, and about one hundred and fifty feet above the mean level of the Pacific. Though the western shore of the lake is but fifteen miles from the beach of the Pacific, the lake drains through the river San Juan, into the Caribbean sea. The lake is deep and unobstructed, and the river, already navigable for light-draught steamers throughout most of its length, requires but little labor to deepen it.

Here, with such a vast water supply at the summit, with the lake itself as a summit level, nature seems indeed to have offered assistance in connecting the oceans. No great engineering difficulties in utilizing the lake are claimed even by opponents of this route. There are no startling propositions connected with the plan. A large dam is to be built in the river San Juan, to back the water in the river up to the lake, but it is a simple matter of known engineering methods. A lock of ex-

ceptional lift is to drop the canal at the west end of the summit level a distance of fifty-two feet. The dimensions and strengths of the parts of this great construction must, therefore, be specially arranged to withstand great strains, but if objection is made to its size it is quite a simple matter to distribute this descent among two or three locks instead of one large one.

It is not to be expected that estimates can be very exact in a great scheme of proposed work, but about these plans there is nothing new or strange. We have here a minimum of unknown quantities. The estimate is about fifty million dollars, and seventy-five million dollars is proposed for capital, but if it cost two hun- million dollars, we have a tonnage in the beginning which will pay six per cent. upon the investment, and the tonnage will increase largely. There can be no doubt that besides the ships now needing the canal, a great additional commerce will be created by the existence of such transit.

The scope of this paper will not permit much discussion of detail, but new advantages appear at each examination of this locality. In the act of constructing the canal we are, at the same time, harnessing and making subservient to our needs a water power of enormous capacity; supply continuous and inexhaustible, with a head of one hundred ten feet of elevation. And this at a point where the products of the world, the raw materials and the manufactured, meet in their passages between Alaska, California, China, Australia, Peru, and Chili on the one hand, and Europe, Africa, and the United States on the other. At a point, too, where the salubrity of the climate, the fertility of the surrounding country, will give favorable chances to great undertakings.

What vast opportunities are here disclosed! What an *entrepôt* for the coffee and sugar of Costa Rica and

Nicaragua, the guano of Peru, the lumber of Alaska, the grain of California, meeting the cotton of our south, the manufactures of the United States and Europe! What factories, mills, ship-building industries may we not see in the near future along the line of the canal and upon the great lake itself!

The mind grows weary in reviewing the long array of possibilities which nature, in its kindest mood, has placed in this favored spot for the use of man.

Referring briefly to the lines in Costa Rica and Honduras, the former connecting Chiriqui lagoon with the gulf of Dulec, and the latter crossing from the bay of Honduras to the bay of Fonseca, they may be summed up by stating that excellent locations for railways were found here, with good harbors at the termini, but that the elevation of the mountain range in this vicinity made canals impossible.

Passing on still farther to the north and west, we come to the last of the isthmuses, that of Tehuantepec.

Cortez satisfied himself with regard to its usefulness as a land transit, and sent by that line much of the equipment arriving from Europe for his Pacific fleets fitting out for exploration and conquest. Later on, when no longer used, the world fell again into ignorance concerning it, and the ancient legends of a strait existing here gained a fresh credence until as late as the middle of the last century. In our own time, the scramble to get to California in 1849 caused it to be used once more, and to the routes known as "around the horn," "across the isthmus," "over the plains," was added this one under the name of "through Mexico."

This route demands at my hands something more than a passing notice, for in our present congress vigorous efforts are being made by Captain Eads to obtain government assistance for a project to carry ships from

ocean to ocean across Tehuantepec, upon a railway. Resting his claims for notice, as does the eminent Frenchman, upon past services of unquestioned merit, Captain Eads advances, as the crown of his engineering career, a scheme which on its face is of doubtful practicability, and which, if proved practicable by the expenditure of vast amounts of money, labor, and ingenuity, can still be shown to be wholly unnecessary. He proposes, as a canal here is impossible, to take seagoing ships, loaded with heavy cargoes, out of the water, lift them upon a cradle, and carry them by rail across six hundred fifty feet of elevation, through swamps and across streams, and finally to lower them into the water on the other side of the isthmus.

The mass of engineering opinion regards the building of embankments, the management of grades and turnings, to be, under this heavy load, difficult and dangerous—perhaps impossible. The mass of nautical opinion considers the lifting and carrying of heavy ships loaded with railroad iron and other heavy weights, to be dangerous in the highest degree to the integrity and safety of the ships' hulls. This gentleman, though, is able, and possesses an ingenious mind. Perhaps he can, at enormous expense, succeed in doing it. But why does he wish to do it? Simply to avoid the breaking of bulk—the discharging cargo and loading cars, the discharging cars and stowing cargo—the two handlings of freight, in fine.

There is more than one way of avoiding this breaking of bulk easier than he proposes. Ships for this isthmus trade can be easily fitted with interior decks on which rails are laid for cars of the lightest and snuggest construction, stowing closely together, and losing but little stowage room by their interstitial spaces. Cargo may be stowed in them, and these cars, of a size to fit a narrow-guage road across the isthmus, can be hauled out

through the bow or stern ports, in a dock arranged to float the ship higher or lower, as needed, in order to bring its decks in succession at the level of the shore tracks

These cars would be run across a cheaply constructed narrow-guage railway, and run into the hold of a ship on the other side of the isthmus, fitted in the same way to receive them.

Some little stowage space would, of course, be lost, but this loss would be slight compared with the enormous tolls each vessel would have to pay to allow dividends on the expensive railway needed to carry bodily a large vessel and her cargo.

I do not claim that this is a specially good project; but only that it is one of many plans which are more feasible, economical, and sensible than Captain Eads' present scheme.

These remarks, to which you have kindly listened, will have shown to you that only three localities have been considered as worthy of being tried: Panama, Nicaragua, and Tehauntepec. You will also have discovered that some persons, myself among the number, believe Nicaragua the only route for efficiency and economy. Did nature, however, offer opportunities for constructing canals at each of these localities, they would all be more or less favorable for the use of commerce, compared with the long and expensive voyage around the cape of Good Hope or the Horn. Tehauntepec would best serve the coastwise traffic which would be established between the gulf of Mexico and our Pacific states. For all other traffic of this country and of maritime nations generally, the more southern routes would be preferable, and between the two, Panama and Nicaragua, Panama would be avoided by a large proportion of the traffic, namely, the sailing ships, owing to the continuous calms which prevail for hundreds of miles to

seaward from that port. The estimate of the amount of tonnage passing through the Nicaragua canal when first opened, was about four millions of tons per year. This estimate, the mean of several reliable calculations by eminent experts, was based upon the figures of the world's shipping trade in 1870. It may now, with justice, be raised to five millions of tons. Upon this tonnage, at the rate of two dollars fifty cents per ton, which is about the rate of toll through the Suez canal, twelve million five hundred thousand dollars would be the gross annual revenue. In the estimates for a Nicaragua canal, five hundred thousand dollars has been allowed for the working expenses annually, and this would leave a net revenue of twelve million dollars with which to pay the interest upon the cost of construction. What that cost would be is known quite closely in the case of Nicaragua. Work will not be started here in the ignorance which marked de Lesseps' beginning at Panama. Careful instrumental surveys have been made, borings have been sunk, both by land and water, to learn the quality of the cube to be excavated, and where obstacles have prevented exact knowledge, the cubes have been estimated for as solid rock. The estimate for a canal at Nicaragua, larger than that at Suez, is about fifty million dollars, and to this fifty per cent. has been added for all contingencies, making seventy-five million dollars.

Though many able engineers believe that it can be built for much less, I believe that sum will represent very closely its total cost. What Captain Eads' project of a ship railway will cost, no one seems to know. His idea is so problematic that no reliable estimate can be formed. Seventy-five millions is mentioned by Captain Eads. Careful surveys will no doubt make the estimate much greater, especially when we consider the costly equipment needed to carry its heavy burdens. But if it cost

only as much as the Nicaragua canal, it is to be remembered that the working expense of railroads is over fifty per cent. of their gross revenues, and of such an abnormal railroad as this is they would probably be much greater. If these twelve and a half millions represented the total revenues, from four to five millions is as much as could be expected for net revenues from a ship railway. Now judging by the small percentage of gross receipts needed for the working expenses of the Suez canal, embarrassed by the drifting sands and burdened by a costly home administration, we may reasonably expect a half million of dollars to cover the annual working expenses of a completed Nicaragua canal, leaving a net revenne of twelve millions, or sixteen per cent. on the cost of construction.

Of the final cost of de Lesseps' sea-level canal at Panama, if there could be anything final about it save utter failure, nothing can be known, except that it will be a fabulous amount. A fresh debt of one hundred twenty millions of dollars has lately been incurred. This loan was offered to subscribers at forty-five per cent. (450 francs for a 1,000-franc bond), but the cost of placing this loan will, it is believed, reduce the amount to thirty-nine per cent.; or to about forty-seven millions for the one hundred twenty millions. It is believed, with good reason, though the debts of the company are difficult to ascertain, that about one-third of this amount is already owing to contractors and others for work already done. So that without considering interest on its enormous obligations, the company will have but a small portion of this new loan to apply to work upon the canal. These obligations now amount to a sum little short of three hundred millions of dollars, and with this huge debt staring them in the face, I can say without exaggeration, that the great difficulties and expenses of excavation are all still before them, and the

knotty, perhaps impossible, problem of the Chagres river is still unsolved.

Do not, I beg you, permit this showing of the Panama canal to influence you against any canal between the oceans. These facts do not surprise those who have studied the question. Great engineers warned Paris and the world of just such a disaster at the Paris congress, while they urged Nicaragua upon their attention, as being an entirely feasible, economical engineering project. We know why they were not listened to; we know how the French clustered loyally about their famous de Lesseps; how he, totally ignorant of the topographic and climatic difficulties, flushed with success and impatient of contradiction would hearken to nothing but a French plan, executed by Frenchmen. Do not, therefore, let this influence your minds against a plan and route recognized as practicable for centuries, and already so closely surveyed as to leave no element of doubt as to its engineering qualities, its small cost, and its final value; for such is indeed a temperate description of the route by way of lake Nicaragua and the river San Juan.

We have now considered the three routes and methods before mentioned, by which the oceans may be connected: Panama, a sea-level canal, Nicaragua, a canal with locks, and Tehuantepec, a ship railway.

By way of the Panama isthmus a canal with locks could have been constructed. It would have been expensive; vexatious problems would have presented themselves in supplying water to its summit level. Nevertheless it was a possible, though not attractive, engineering problem.

Attempts have been made, however,—disastrous attempts—to construct a canal at the level of the sea at this isthmus. This project, impossible as an economy, impracticable as an engineering scheme, has, by its

failure, made it improbable that a canal of any kind can, during this century, be made successfully at Panama.

The route for a canal with locks through Nicaragua, using the lake as a summit level, presents itself most favorably, both as an engineering and an economical problem ; a scheme which, as far as we can judge, seems specially favored by nature.

The line by way of the isthmus of Tehuantepec, if a canal were possible there, would commend itself to the commercial interests of the United States on account of its northern location, making the transit between our gulf and Pacific states a most convenient and speedy one. So far as is known this water transit cannot be provided. The ingenious conception of a famous engineer may perhaps have there a practical trial, and an effort may be made to carry loaded ships of the largest size on a railway more than a hundred miles long, and which achieves over six hundred feet of elevation in its passage between the oceans.

The ship-railway project is born of a keen desire to utilize this isthmus of Tehuantepec for commerce. It does not arise from any manifestation of nature in favor of its use for purposes of a transit route, More than this, the world generally has not asked for a railway, but for water transit—for a canal.

To those, then, who, like myself, are assured of a Nicaragua canal in the future, it may be of interest to consider it with reference to the United States. We have spoken of its importance to our commerce. Let us now glance at its value from a military and naval standpoint.

From a point of view, strategic and political, it may be said that if this canal were the southern boundary of the United States, our need to hold it would be overwhelming and unquestioned. To permit a feeble race of people with an uncertain government, such as occupy

almost all the western hemisphere south of this country, to control a boundary canal, would soon result in the swallowing up of that feeble nation and of the canal control by some European power, strong and aggressive. Such joint possession as happens with a part of the St. Lawrence river, where another great nation owns the other bank, would not be practicable, if it were Nicaragua or Costa Rica confronting us there. Were the Rio Grande a great channel of navigation, connecting our eastern and western states, instead of the unimportant stream it really is, we could not permit even our neighbor Mexico to have a part in its control. And what is true in this supposed case is the more so when, in reality, between us and the proposed canal there lie intervening countries, all of them feeble and liable to be easily dominated by an outside power.

There is, in fact, no locality favorable to an interoceanic canal which could be any thing but a passage, a narrow thoroughfare, connecting two of our great divisions, our Atlantic and Pacific states.

Further, we are pledged by our traditions to protect the states of Mexico and Central America from European aggression. It is plain that we must abandon those traditions if we are not to control a great artificial channel penetrating the very heart of Central America, and passing from sea to sea.

These reasons for our holding the canal would apply in the case of one along any practicable line, but much more in the case of a Nicaragua canal, for if that route be followed the construction of the canal at once establishes in the lake, in addition to the water transit between the oceans, a grand interior fresh-water harbor within a few hours of either ocean. As a base from which to dominate and control both coasts and the West Indies, the strategic value of such a harbor is beyond estimate. Absolutely sheltered and secure, its fresh

water constantly tending to cleanse the ships' bottoms, its lofty islands offering such sanitary opportunities that no tropical sickness need ever prevail, this great lake, with anchorage for the world's fleets, seems to thrust itself upon the attention of intelligent engineers as the only solution of the transit problem; while to the naval officer, who sees in the near future the necessity for the United States to control with its fleets the coasts and the great archipelago to the south of us, this capacious interior basin offers itself as harbor, depot, strategic base, making such control possible and simple.

It seems idle to argue as to whether it be wise or expedient to obtain this domination. Whether right or not, great nations always do control affairs of the feeble and unprotected in their vicinity. Whether we seek it or not, this domination will be forced upon us in the south throughout those regions and seas which lie near us.

Nature has defined the limits of such areas as should, from duty and policy, be of special interest to us. The Caribbean sea, gulf of Mexico, West India islands, the shores, east and west, of Mexico and Central America, and the Spanish main,—these must be cared for by fleets in peace, and fought for by fleets in war. Beyond this our influence and interest, physically and geographically speaking, need not extend. Except its north shore' South America is quite removed from us and our interest. Pernambuco and Rio de Janeiro are nearer to Spain and Portugal than to New York and Pennsylvania in actual distance, as well as in language and sentiment. We need not dwell upon this question. A careful inspection of the map of the western hemisphere forces the conclusion upon us that a nation occupying the present position of the United States must, if it lay claims to greatness, be dominant in the Caribbean, the gulf of Mexico, and the neighboring islands and shores.

These are the passage ways, if not, some day, the ultimate destinations, of the richest products of our industry, floated southward from our great central region, and passing through Mobile, New Orleans, and Galveston to the sea. Duty and interest, then, seem to demand that we prepare for this control in the future. It is more than a consequent of greatness, it is greatness itself; it is part of the definition—we cannot be a nation of the first rank while lacking the control of the seas and coasts immediately south of us.

From a naval and military point of view, therefore, the direct advantage of holding such a great base of operations as lake Nicaragua is immense—is, perhaps, when we consider all the circumstances, without parallel in history. If we consider the unhealthiness of the harbors, coasts, and navigable rivers of the Caribbean region, and, on the other hand, the comparative immunity from disease to be enjoyed by a fleet occupying the elevated waters of this fresh-water lake, with hill slopes on its islands reaching far above the yellow-fever line; if we note the rapid destruction of iron ships' hulls in sea-water, the alarming fouling of barnacles and grass, and consequent serious decrease of speed, frequently reducing a fourteen-knot steamer to eight knots; and if we then reflect upon the quick remedy which fresh water always affords in this difficulty; if we consider the admirable strategic position of the lake, and regard its size and depth—so great as to permit the largest fleet to drill itself to the highest evolutionary efficiency;—these and numerous minor details, if well considered, will not fail to convince us of the value of this great possible depot and station.

A well-appointed dockyard would be established on the shores of the lake, or on its lofty island of Ometepee. Hospital sites and camping-grounds for the crews of vessels would be selected close to the fleet's anchorage,

but well above the fever line, on the mountain slope, in a bracing and healthy air. Store-houses and hulks, coal piles and elevators, would give facilities for the rapid coaling and provisioning of the fleet. Stone dry-docks alongshore, and floating docks sent from the United States in sections to be put together on the lake, would offer opportunities for the quick repairs of damages sustained in battle. Telegraph cables would connect the station with Washington, and railways through Mexico, always available in peace, would be easily made so in any war against European powers. It is well to note here, as an important item, that such a government establishment, always kept ready for a war, would not, during the long intervals of peace, be expensive. The nautical needs of the merchant marine are so nearly those of men-of-war, that a dockyard of the first class, with all its repair shops and provisioning facilities, could be kept fully employed and in a high state of efficiency during a peace however long; and this at no expense, no running expense, to the government, but, on the contrary, at a handsome annual profit.

Here, then, in this secure, capacious, and healthy retreat, within a few hours of the open oceans, let us see what would be the capacity for reaching out possessed by a fleet in this stronghold. If the speed of a fleet be fifteen knots, it can in two and one-half days, from the canal entrance, reach the Yucatan channal, south coast of Cuba, windward passage, Jamaica, and as far east as Maracaibo on the Spanish main. In five days it may be off the mouths of the Mississippi and Rio Grande, in the Florida strait or among the Bahamas, in the Mona passage or at Martinique and Barbadoes, and include in its reach to the eastward the whole of the Spanish main.

On the Pacific side, in five days, at full speed from the entrance of the canal, the fleet can arrive in the

mouth of the gulf of California in one direction, or upon the coast of Peru in another.

It will thus be seen how long would be the arm, how effective the power of a swift and well-conditioned fleet, ready to act on either coast, and drawing constant strength and nourishment from this admirable lake base. With a strong naval force in Hampton Roads, another in California, ready to move effectively at a moment's notice, then a similar fleet in lake Nicaragua would complete what may be called the naval strategic defence of our nation. There would be many additional details in any complete scheme of defence. Key West must be held, and the mouth of the Mississippi protected; a strong force, auxiliary to the Hampton Roads fleet, must hold the sounds and channels of Long Island and Nantucket; Puget sound must be held, and the gulf of California dominated.

The three great divisions of our fleet, to protect our coasts and our vital interests to the southward, must, however, hold positions similar to those above mentioned, in a large and well-considered scheme of strategy.

It would be possible for the lake Nicaragua force to join the Hampton Roads fleet and engage an enemy off Havana, and thence, allowing two days for coals and provisions in the lake, it could join the California fleet off cape St. Lucas, and fight an enemy in the gulf of California, and this within twelve days of the first battle.

Let me ask your forbearance for a few moments longer, while I quote some sentences from a remarkable pamphlet written in 1846 by Louis Napoleon, afterwards emperor of the French. It is as follows:

"The geographical position of Constantinople is such as rendered her the queen of the ancient world. Occupying as she does the central point between Europe, Asia, and Africa, she could become the *entrepôt* of the

commerce of all these countries and obtain over them an immense proponderance ; for in politics as in strategy, a central position always commands the circumference. This is what the proud city of Constantine could be, and this is what she is not, because as Montesquieu says, 'God permitted that Turks should exist on earth, a people most fit to possess uselessly a great empire.' There exists in the New World a state as admirably situated as Constantinople, and we must say up to this time as uselessly occupied. We allude to the state of Nicaragua. As Constantinople is the centre of the ancient world, so is the town of Leon the centre of the new, and if the tongue of land which separates its two lakes from the Pacific ocean were cut through, she would command by virtue of her central position the entire coast of North and South America.

"The state of Nicaragua can become, better than Constantinople, the necessary route of the great commerce of the world, and is destined to attain an extraordinary degree of prosperity and grandeur.

"France, England, and Holland have a great commercial interest in the establishment of a communication between the two oceans, but England has more than the other powers a political interest in the execution of this project. England will see with pleasure Central America becoming a powerful and flourishing state, which will establish a balance of power by creating in Spanish America a new centre of active enterprise, powerful enough to give rise to a great feeling of nationality, and to prevent, by backing Mexico, any further encroachments from the north."

These utterances of a man of thought, this evident fear of the eagle in the north, are significant. To this student of large enterprises, the pre-eminence of lake Nicaragua, politically and commercially was quite plain, orty years ago. He could not think that the eyes of

this eagle would lack the proverbial keenness of vision, that the great republic would have that political control pressed upon it, that commercial power freely offered to its citizens—all of it only to be rejected with cold indifference.

What is it we are doing my friends in rejecting this control? Are we blind to the strides the Germans are making toward commercial supremacy in Mexico and Central America? Is there no significance in the loans which English capitalists are freely offering to Nicaragua to improve the navigation of her river and lake? These things tend but one way. The English merchant, the German chancellor, the French engineer—they know what these things mean; they know what their nations need for their development. It is only we that do not know. It means empire, ladies and gentlemen—the control or possession of this canal means empire. It is to our wealth, our development, our supremacy as a nation among nations, what India, and more than India, was to English merchants, and to the English crown and nation. It means the guiding of the great Pacific's wealth into New York rather than Liverpool, into New Orleans instead of Marseilles. And we,—we also will learn this some day, when, alas! some other nation has seized the golden key as it drops from our listless hand and with it has unlocked for itself the door to wealth, fame, and power; when another nation has built and holds the Nicaragua canal; then will we learn and know, and then, the hand that dropped the key must grasp the sword in its place and so win back the key—and again will precious blood and treasure be wasted in long wars to regain that which with slightest effort and to our great profit we might now peacefully retain.

Surely our commercial public should be anxious to secure for themselves the building of this canal in the interest of trade. Surely our national government

should be quite as urgent to hold a controlling interest in it, as a pillar of strength in peace and in war.

Nicaragua has cordially offered to our government canal rights of inestimable importance. It is not for us to know what wise reasons caused our government to decline this offer. Nicaragua has now and for years offered to certain of our citizens a liberal concession for the construction of a canal. These citizens are to-day unable to accept the offer, though anxious to do so, because the fatal apathy of our capitalists and merchants denies to this project the assurance of financial support. So blind are they to the immense profits accruing from this project, both to themselves and to the country, and yet so keen to see smaller gains in smaller and less secure enterprises, that we are driven to believe at last the old story of the man who saw with ease the flies on the barn door, but could not discern the door itself.

Fortunately, light is now dawning, and there is good reason to expect that the next few months will see American citizens of wealth and reputation entering upon this great project, and identifying themselves with this, the greatest of peaceful achievements known to our century.

Let us hope that it will come by peaceful means, that in this instance "grim-visaged war" will not enter into the problem, but that the great canal may draw nations closer together in the bonds of peaceful trade and of enlightening commerce. Let us think of it as doing for us what we so sorely need, building up for us a vast coastwise traffic of sea-going ships between our Atlantic and Pacific coasts, leading surely to a new birth of that great American shipping which died in our civil war. Let us, looking hopefully into the future, see the canal bringing nearer to us the brave republic of Chili and its neighbors, see it binding Australia and New Zealand to England, Manilla and the Philippines

to Spain, see it, in fact, bringing the whole Pacific close to our doors and to Europe, and making most true and forcible the old motto of ocean commerce, that

"The seas but join together
The nations they divide."

TEHAUNTEPEC SHIP RAILWAY (proposed)—
Length, 134 miles.
Probable cost of construction, $100,000,000.
Probable receipts (gross) $12,500,000 ; (net), $6,000,000.

NICARAGUA CANAL (proposed)—
Length (canal) 40 miles ; (river and lake) 130 miles.
Locks, 7.
Floor of canal, 80–120 feet wide.
Surface of canal, 80–300 feet wide.
Depth, 28 feet.
Probable cost of construction, $100,000,000.
Probable receipts (gross), $12,500,000 ; (net), $12,000,000.

SUEZ CANAL (completed)—
Length, 99.9 statute miles.
No locks—à niveau.
Floor of canal, 72 feet wide ; surface, 190–328 feet wide.
Depth, 25 feet.
Tonnage using the canal in 1883, 5,775,861 tons.
Receipts from tolls during 1883, $13,702.413.
Cost of construction, $93,000,000.

PANAMA CANAL (constructing)—
Length, 46 miles (statute).
No locks—à niveau.
Floor of canal, 72 feet wide ; surface, 100–164 feet wide.
Depth, 28 feet.
Probable tonnage, if canal is completed, 5,000,000 tons.

NICARAGUA SHIP CANAL
LAKE NICARAGUA
RIO SAN JUAN
PACIFIC OCEAN
ATLANTIC OCEAN
COLORADO RIVER

Probable receipts, if canal is completed, $12,500,000.
Probable cost of construction, $500,000,000.

SAVING IN DISTANCE AND TIME BY ISTHMUS CANAL.

	Miles.	Gain for Sailing Ship—Days.	Gain for Freight Steamer—Days.
New York to Hong-Kong,	2,450	27	12
" Yokohama,	4,200	40	21
" Callao,	4,390	52	22
" Honolulu,	7,100	67	35
" San Francisco,	7,370	72	37

APRIL 28, 1886—FORTY-EIGHTH REGULAR MEETING.

In the absence of the chairman—Dr. Warring: L. C. Cooley, Ph.D., presided.

The meeting was adjourned to May 4, at which time the following gentlemen were elected officers of the section for 1886–1887:

Chairman, . . WILLIAM G. STEVENSON, M.D.
Recording Secretary, . MR. CHARLES N. ARNOLD.

TRANSACTIONS

OF THE

SCIENTIFIC SECTION,

1886–1887.

DECEMBER 1, 1886—FORTY-NINTH REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding; twenty members and fifty guests present.

EARTHQUAKES.

A PAPER READ BEFORE THE SCIENTIFIC SECTION.

BY WILLIAM G. STEVENSON, M. D., CHAIRMAN.

Members of the Section, Ladies and Gentlemen: The general interest which the recent destruction of Charleston has again aroused in relation to earthquakes, must be my excuse for presenting to you this evening a brief *résumé* of some of the theories and facts relating thereto.

For such facts or information as this paper may contain, I am entirely indebted to the general literature on the subject, and in a special manner to the recently published work of Prof. Milne on *Earthquakes*, which I have followed and from which I have freely quoted.

Notwithstanding the fact that in all ages earthquakes have been among the dynamic agencies in nature which have produced the most terrible effects upon man, it is only in very recent years that their phenomena have been studied with any special precision, and even now but little is known as to their true origin.

When we recollect, however, that the inductive basis of our knowledge relating to the nature and causes of earthquakes, rests upon our knowledge of chemistry, geology and physical philosophy, which are practically the creations of the present century, it is less strange, than it might at first appear, that we possess so little accurate information about these convulsions of the earth.

Passing over the fanciful explanations of many ancient writers relating to the motion of earthquakes, we find that Travagini, in 1679, first entertained the thought that an "earthquake was a pulse-like motion propagated though solid ground." Hooke, in 1690, divided the phenomena into different genera according to the effects produced in elevating, depressing, transporting or transforming the earth's surface or crust. Woodward, in 1695, thought that earthquakes followed from the contact of water with fire in the interior of the earth. Priestley connected them with electrical phenomena, while Mitchell, in 1760, thought they came from the impact of a subterranean liquid mass, breaking in waves upon the thin, flexible crust of the earth, which was contorted and thrown into folds as a "carpet is thrown into undulations when shaken."

Mitchell did not, however, comprehend the true principles of wave motion, and hence could not satisfactorily explain the reason why large areas were almost simultaneously disturbed.

It remained for Sir Thomas Young, in 1807, to indicate that the real nature of earthquake motion was vibratory, and was "propagated through the earth nearly in the same manner as a noise is conveyed through the air."

To know the true nature of earthquake motion is of first importance—for it is only as we are able to trace these phenomena to their starting place, that we are able to formulate a theory as to their producing cause.

In obtaining a clear idea, therefore, of vibratory motion, and in proving that earthquake motion is of this character—the foundation was laid for the more complete unfolding of the idea advanced by Young—which Robert Mallet, in 1846, formulated into a definition which described an earthquake to be—"the transit of a wave or waves of elastic compression in any direction from vertically upwards to horizontally, in any azimuth, through the crust and surface of the earth, from any center of impulse or from more than one, and which may be attended with sound and tidal waves, dependent upon the impulse and upon circumstances of position as to sea and land."

To the waves of elastic compression and extension—which may be designated the rectilinear motions of the molecules—Prof. Milne adds, what he calls the waves of elastic distortion—which may be regarded as the twisting or contorting motion of the masses—as essential to a complete definition of earthquake motion.

These elastic waves doubtless exist—but they are seen only by the "mind's eye," and are known by processes of reasoning and not by direct experimental evidence. "A true surface undulation," like a water-wave, is the only kind of wave motion which thus far responds to the tests applied.

Seismology—or that department of physical science which relates to the study of earthquakes—has, largely through the labors of Mr. Mallet, made much progress during the last forty years towards solving the complicated questions pertaining to earthquake phenomena.

In the experiments which have been made by Milne and Gray to determine the effects produced in the earth's crust by falling weights—it is found that two distinct sets of vibrations are caused. "In one set—the normal," says Milne,—"the direction of motion was along a line joining the point of observation with the point from

which the disturbance emanated; in the other—or transverse—set, the direction of motion was at right angles to that line."

It is also found that a hill does not cut off the vibrations to any large extent, although their direction is mostly transverse as they ascend its side; a pond of water, on the other hand, completely cuts off the vibrations, which creep gradually around its margin.

The transverse vibrations continue for a longer time.

From the report of General Abbot relating to the explosion at Hallet's point, and the observations of Mr. Mallet in his experiments at Holyhead, we learn that "the transit velocity increases with an increase in the intensity of the initial shock." And a higher velocity is attained when an explosion occurs under deep, than under shallow water.

General Abbot found that "a high magnifying power of a telescope is essential in seismometric observations"; that the velocity of transmission is higher in proportion to the violence of the initial shock, and "diminishes as the general wave advances"; that "the movements of the earth's crust are complex, consisting of many short waves first, increasing and then decreasing in amplitude; and with a detonating explosive, the interval between the first wave and the maximum wave is shorter than with a slow burning explosive."

Observations taken at the time of earthquakes indicate that motion may vary in its direction, and that the amplitude of the earth's movements rarely exceeds three or four millimeters; and when it is beyond the elastic limits of the material, fissures in the earth are formed which often open and close with the vibratory movements.

The condition and nature of the underlying soil in a measure determines the intensity of the shock, or may neutralize it entirely. The wave, in passing from one

bed of rock to another of a different character, is partially or wholly reflected; and, in proportion to the amount of reflection or interference, is the shock lessened: some small portions of earth being free from any shock or movement, while all around the vibrations are distinctly and destructively felt.

It is said that "the Temple of Diana at Ephesus was built on the edge of a marsh, in order to ward off the effect of earthquakes;" and we are told by Pliny that the Catacombs have protected the Capitol of Rome; while others state that the Romans discovered the protecting influence of caverns, deep wells, and quarries against earthquake violence. There is no doubt but the principle therein involved is a correct one. The vibrating wave-impulse is interrupted, because the continuity of the medium through which it passes is broken, and the force of the impulse is spent upon the margins of the caverns or ravines the same as it would be upon the face of a cliff or scarp were it in the course of the vibratory movement. The points of wave-interference are the places of greatest shock and danger. The laws of motion are the same whether they apply to air, water, or earth-waves.

The duration of an earthquake varies from a minute to many days, or even months. "In Japan, A. D. 745, there was a shaking which is said to have lasted sixty hours; and, in A. D. 977, there was a series of shakings lasting three hundred days." Prof. Milne further reports that at San Salvador, in 1879, more than six hundred shocks were felt within ten days; in 1850, at Honduras, there were one hundred eight shocks in a week, and two hundred shocks in a single day at Lima, in 1746; and two hundred eighty-three shocks were felt in ten hours, at St. Thomas, in 1868.

The number of shocks experienced at Charleston, August 31st of this year, is, therefore, in no way ex-

ceptional; although the extent and severity of the Charleston earthquake exceed anything heretofore experienced in the United States. It affected an area of nine hundred thousand square miles.

To speak of the effects of earthquakes on land and water is to recite the story of the destruction of Charleston, South Carolina, last August; of Port Royal in 1692; the immense and frightful cleavage phenomena during the Calabrian earthquake of 1783—when the earth opened in fissures one hundred feet wide and two hundred feet deep; in 1158 an earthquake felt in England caused the "drying up of the Thames so that it could be crossed on foot even at London"; coast lines have been changed; mountains have been thrown down; "valleys have been filled; cities have been submerged or buried," and human lives have been destroyed by thousands.

In 1868 when St. Thomas was shaken the sea receded; at Lisbon it rose in a mighty wave sixty feet in height and fell with destructive force upon the city already shattered by the terrible force of the land vibrations. When Lima was destroyed in 1724, a sea wave eighty feet in height poured over Callao.

When Callao and Lima were again destroyed in 1746, the sea rose to eighty feet in height and came rushing over the towns seventeen hours after the land shocks had passed away.

In 1737, on the coast of Lupatka, the sea rose above its level to the immense height of two hundred ten feet.

These sea waves extend their influence over vast areas. Thus the great earthquake of Lisbon sent waves across the Atlantic to our shores in nine and a half hours. The wave of 1868—destroying twenty five thousand lives on the coast of South America—extended over the entire Pacific, traveling at the average rate of five hundred eleven feet per second.

Fortunately such sea-waves are not the common attendants of earthquakes, for, out of fifteen thousand earthquakes observed along various coast lines, there have been but one hundred twenty-four sea-waves produced thereby.

Although the shock is felt so distinctly on the earth's surface, it is the testimony of many observers that, deep underground, it is either not felt at all, or, if felt, it is generally very feeble. Such has been the evidence of those who have been in deep mines at the times when severe earthquakes have occurred.

The explanation of this is thought to be either because of a "smaller amplitude of motion in the solid rocks beneath the surface as compared with the extent of motion on the surface, or else the disturbance is, at a distance from its origin, practically confined to the surface."

The depth at which earthquakes originate varies—it is believed—from a mile and a half, as in the earthquake of Yokohama in 1880, to fifty miles as in Owen's valley earthquake in 1872. Mallet's calculations, however, give the limiting depth at thirty miles, but his calculations were based upon the idea, first propounded by Mitchell in 1700, that "the impulsive effect of an earthquake has an intimate relationship with the height of neighboring volcanoes, the column of lava supported on a volcanic cone being a measure of the internal pressure tending to rupture the adjacent crust of the earth."

Prof. Milne, however, shows that this argument is subject to such qualifications, that it cannot have a general application. He says that the connection between particular earthquakes and volcanoes is not always specially apparent; and, while the column of liquid lava in the cone of a volcano may "measure the pressure upon the crust of the earth in the immediate vicinity of the cone," it may not be a correct index of the pres-

sure originating in a general subterranean liquid mass. Thus, for example, in the crater of Mauna Loa the lava stands "ten thousand feet higher than in the crater of Kilauea, only twenty miles distant."

We may admit that volcanoes act as safety-valves for the pent-up telluric forces, and, by their sudden explosions, become true causes of certain earthquakes, without accepting, to its full extent, the common belief that there is a synchronism between volcanic eruptions and earthquakes.

Some of the great earthquakes of record have not only occurred at times when no volcanic phenomena have taken place, but they have occurred in places where volcanoes did not exist, as at Calabria, Lisbon, and Charleston.

On the other hand, that seismic and volcanic phenomena are often seen together is well established by the evidence given by Herculaneum and Pompeii. When Concepcion felt the great shock in 1835, there occurred two great submarine eruptions. When Riobamba was destroyed in 1797, Mount Pasto, one hundred twenty miles distant, suddenly ceased its activity, while volcanoes within the immediate vicinity of Riobamba were in no way affected. In 1861, a volcano at the base of which Mendoza is situated burst into eruption, when this city, with ten thousand of its people, was destroyed by an earthquake.

Although many earthquakes have their origin in mid-ocean, it is, nevertheless, true, generally speaking, that they are more frequent in volcanic and mountainous regions, along ocean boundaries. And, while earthquakes and volcanoes may be but different effects of a common cause, the evidence at hand does not enable us to affirm that their phenomena have at all times any direct connection.

Following Prof. Milne it may be stated that, as the re-

sult of modern inquiry, earthquakes are to be attributed to many causes acting in such a complex manner that, as yet, it is impossible to determine the exact dynamic value of any one cause in their production. It seems to be well established, however, that "telluric heats, solar heat and variations in gravitating influences" are primary, and often the immediate cause; while, as dependent or secondary causes, we recognize "expansions and contractions of the earth's crust, variations in temperature, barometrical pressure, rain, wind, the attractive influences of the sun and moon in producing tides in the ocean or the earth's crust, variations in the distribution of stress upon the earth's surface caused by processes of degradation and the attractions in the position of isogeothermal surfaces."

To one accustomed to the use of microscopes or telescopes, it is well known, from many unpleasant experiences, how small a cause, comparatively speaking, is required to produce earth tremors which seriously interfere with the accurate use of these instruments.

When reflecting a star in a tray of mercury at the Greenwich observatory, it has been found that the tread of people, from London to Greenwich park, caused earth tremors which so disturbed the surface of mercury as to render it impossible to complete the observations until these earth tremors were overcome by mechanical means.

Prof. Paul, when making a survey at Washington to find a suitable site for our naval observatory, found that a passing train of cars caused earth tremors which were readily noticed at the distance of a mile; while Lieutenant-Colonel Palmer, when at New Zealand making observations on the transit of Venus in 1874, found the vibrations of the earth, caused by a railway four hundred yards distant, so disturbing that he was obliged to intrench his instruments in pits three and a half feet

deep in order to escape these artificial tremors, which affected the earth through a coarse, pebbly gravel to nearly this depth.

The observations of M. d'Abbadie and of George and Horace Darwin have abundantly demonstrated that natural earth vibrations are incessant and universal. It is also well known that at times there are large wave-like undulations which travel over the earth's surface and mark their progress by the disturbance of the levels of lakes and seas. These movements or undulations are known as earth pulsations.

That a body as dense as the earth and of its magnitude should throb and pulsate as if it were a thing of life, seems hardly possible; but, when we measure the forces which constantly operate within it, and act upon it from without, we learn that its magnitude and density are great or small only in a relative sense; its rocks are more resisting than its water, but yet when the tidal forces of nature play upon the rocks they are twisted, contorted and broken as if they were glass toys in the hands of a giant.

Barometrical pressure alone has been shown by Mr. George Darwin sufficient to depress a continent. On the assumption that the earth has a rigidity like steel a "barometric rise of one inch over an area like Australia, gives a load sufficient to sink that continent two or three inches."

The tides have been shown sufficient to cause our shores to rise and fall with their ebb and flow.

We feel assured, therefore, that known forces are sufficient to cause earth tremors and pulsations; and it seems but reasonable to ascribe to these same or kindred forces—when acting with greater intensity and power—the causes which, under favorable conditions, produce all the phenomena of earthquakes. Yet, as a matter of fact, it is found that what we may call external causes—

like solar and lunar attractions and barometric fluctuations—are much less important agents than those which belong more directly to the structure and conformation of the earth itself.

We shall not, therefore, discuss the influences which may be exerted by rain, winds, and changes in atmospheric temperature in the causation of seismic phenomena, because their influence is not preponderating. But reference will be made to some of those influences which are known to possess more direct seismic potency.

Mr. Poulette Scrope ascribed most earthquakes to "the snap and jar occasioned by the sudden and violent rupture of solid rock-masses, and, perhaps, the instantaneous injection into them of intumescent molten matter from beneath."

Mr. Mallet, however, argues that, on mechanical principles, such rock fractures "could produce only very weak impulses;" but, nevertheless, thinks that some earthquakes "may be due to the movement and crushing of rock-masses by tangential pressures, produced by secular cooling of the earth."

Whatever may be the agencies or causes which produce land elevations and depressions by which, from undue strain or tension, cracks or faults are caused in the bed-rocks of the earth, may be regarded as seismic causes also; for faulting cannot take place without causing shocks which can be felt, and whose severity will be measured by the magnitude of the rock-fracture and dislocation.

In minor earthquakes, the fractures do not extend to the surface; and so a flexure, rather than a marked displacement, only may be observed. When the fracture and displacement are beneath the ocean, of course we can only feel the shock, or observe the water-wave; but cannot discern topographical changes, except in the lifting up of islands or coast lines, or in their depression.

"By the earthquake of 1839 the island of Lemus was suddenly elevated eight feet." And, during the earthquakes of Concepcion, in 1835, the coast was lifted several feet above the sea-level. While, by the Owen's valley earthquake, in 1872, a fault was produced forty miles long and from five to twenty-five feet in height.

Wherever rock-fractures and displacements have taken place, there can be no doubt but they have resulted from the action of seismic forces.

Humboldt saw in "the reaction of the fiery interior of the earth upon its rigid crust" a cause sufficient to explain seismic and volcanic phenomena. Some have suggested that this "reaction" consists in the sudden outbursts of steam beneath the earth's crust, and its escape through cracks and fissures.

Whether steam accumulates by "separating out from the cooling interior of our globe" or results from the "percolation of water from the surface of the earth down to volcanic foci," there can be no doubt but its expansive force is capable of causing a sudden explosion, and thereby be a veritable cause of seismic phenomena. Fissures beneath the ocean—where the most destructive earthquakes have arisen—may "admit water to volcanic foci;" the intense heat to which it is submitted will, for a time, hold a portion of the water in a spheroidal state, during which time tremors may be felt, and then when, from the spheroidal state the water is converted into steam, its expansive force causes explosions which are felt as earthquakes.

Again it is said that the evisceration which takes place on such an immense scale during volcanic action—when lava is ejected in such quantity that in some individual cases it has exceeded the magnitude of Mount Blanc; or on a smaller scale by the mining excavations in our coal fields, and mineral mines, and from the outpouring of petroleum oil, and gas, whereby subterranean hollows

are produced which render the earth unstable—it is said that such evisceration causes earthquakes from the collapse of the large hollows thus produced.

Observation, however, does not find the evidence which supports this theory to its full extent; and yet when we add to the evicerating agencies already mentioned the one yet more powerful and constant in its action, viz.: chemical degradation, we are obliged to recognize an agency which may at least be sufficient to cause seismic shocks of local import.

Because of the known power exerted by the attractive influence of the sun and moon in causing the tides in our oceans, some have thought that this same influence is sufficient to cause, what may be called, elastic tides in the earth's crust, and also tides in the molten liquid within the interior of the earth, which, acting upon the earth's crust, produces fractures and faulting with the attendant phenomena of earthquakes.

Falb elaborated this idea and held that the internal fluids were drawn, by the attraction of the sun and moon, into cracks and channels which are in the crust of the earth, and are therein cooled, or exploded and by their explosion cause seismic and volcanic disturbance.

The attractive force of the moon upon the water on our planet is sufficient to "lift a hemispherical shell eight thousand miles in diameter about two or three feet higher at its crown than it lifts the earth," and, from the investigations of Lamé, Darwin and Thomson, we are asked to believe that this same force is sufficient to produce enormous elastic tides. If it be true that the interior of the earth is fluid, the elastic tides "would be sufficient to lift the waters of the ocean up and down so that the oceanic tides would be obliterated." Such a condition does not exist and we are therefore interested to know—as a preliminary point in our discussion—

whether the interior of the earth is really fluid, viscous or solid.

On this question there is a diversity of opinions. It is generally admitted that the "density of the crust beneath the mountains is less than below the plains, and still less than that below the ocean-bed," and that there is an "excess of heavy materials in the water or southern hemisphere and beneath the ocean-bed as compared with the continental masses;" it is because of this condition—some have said—that the vast body of water is held in the southern portion of the globe; hence, too, it is argued the interior is composed of heavier material than the surface, and may be metallic.

Pressure, however, would make the nucleus heavier even if it were composed of the same—or no heavier material than the crust—unless indeed the effect of pressure is neutralized by some other agency; and that is the only force capable of thus acting. The structure of the earth's crust clearly indicates that the heavier substances are towards the center, and it is likewise well established that an excess of heat exists below the surface also.

The fact of internal heat, greater than the surface temperature is proved by the hot vapors, ashes, molten rocks, water and fire which are thrown out of volcanoes; from thermal springs, and from the average rate of increased heat—1° Fahr., for every fifty or sixty feet descent—obtained in artificial borings, wells and mines—which, at fifty miles would give a temperature of 4600°, which is greater than the fusing point of platinum.

From such data geologists have urged the inference that the earth's interior is a molten mass, whose heat is slowly escaping outward by the processes of radiation, conduction and convection.

The earth therefore is constantly losing heat, and, as

a cooling body, it is constantly shrinking. The surface temperature, however, remains nearly uniform—being largely regulated by the solar heat received, by the amount of heat radiated into space and by atmospheric influences,—so that the contraction of the exterior is more gradual than that of the interior portion of the globe. This process of contraction causes heat, and thus feeds the internal fires ; but as the internal portions contract more rapidly than the external portions, they shrink away from the external shell and leave hollow places, like a shell within a shell—with a hollow sphere intervening. The strength of the outer shell is not sufficient to withstand the gravity of its own rocks—and it breaks in here and there—with earthquake shocks—and leaves fractured and distorted rocks as evidence of its fall. Faulting thus causes earthquakes. Such are some of the geological data and inferences relating to our globe.

In opposition to the theory of the internal fluidity of the earth, are some physical and astronomical arguments which have commanded much attention and gained the support of many of our ablest physicists and geologists.

Mr. Hopkins in 1839 contended against the liquidity of the earth's interior, because, such a condition is, according to his calculations, incompatible with the planetary motions of precession and nutation. These movements cannot possibly occur as they do if the earth consists of "a central ocean of molten rock surrounded with a crust of twenty or thirty miles in thickness," His calculations required a crust not less than eight hundred or one thousand miles in thickness in order to permit of an explanation of these present motions.

M. Delauney reasoned that "if the interior were a mass of sufficient viscosity," it might act as a solid and

not interfere with the phenomena of precession and nutation. But Sir William Thomson showed that this theory "breaks down when tested by a simple calculation of the amount of tangential force required to give to any globular portion of the interior mass the precessional and nutational motions which, with other physical astronomers, he attributes to the earth as a whole."

Sir William Thomson has re-examined this problem and gives as his opinion (I quote from Geikie) that the solar semi-annual and lunar fortnightly nutations absolutely disprove the existence of a thin rigid shell full of liquid. A thin crust requires a rigidity which is not possessed by any known substance, and a crust less than two thousand or twenty-five hundred miles in thickness could not bear the strain of the tide-producing force of the sun and moon, and maintain the earth's configuration. He concludes that the mass of the earth "is on the whole more rigid certainly than a continuous, solid globe of glass of the same diameter."

This view, which Thomson has again reviewed and modified, is strengthened by the mathematical investigations of Mr. George H. Darwin on the bodily tides of viscous and semi-elastic spheroids and the character of ocean tides on a yielding nucleus, whereby he shows that "no very considerable portion of the interior of the earth can even distantly approach the fluid condition."

Seeking to reconcile the geological facts "which seem to demand the existence of a mobile mass of intensely hot matter" beneath the earth's surface, with the requirements of physics, Mr. Fisher suggested the existence of a fluid or viscous material lying between the external crust—which is solid by cooling, and an intensely hot but solid nucleus which is rigid by pressure.

What the entire truth is concerning these very important questions, is a problem for future solution;

nevertheless, it seems to me that geology has furnished abundant evidence, in the fissures and faults of the surface rocks, to justify an inference as to the nature and origin of the mighty blow which sends an impulse over a continent, and makes the earth tremble as though it were a reed shaken by a storm.

In an interesting discussion which followed this address Prof. W. B. Dwight reviewed the many and various phenomena which must all be explained by any adequate theory of the origin and nature of earthquake motion. He gave reasons for believing that though many of the less important local earthquakes are doubtless due to volcanic action, the chief cause of important and extended earthquakes is a displacement of the strata by fracture due to contraction of the crust of the earth. As a condition to such contraction from cooling, there may be a plastic zone of molten rock at some distance below the earth's surface. Sir William Thomson has withdrawn some of the extreme conclusions which he had arrived at in opposing the theory of the possibility of a molton zone; and the arguments based on mathematical calculations which he still uses against this theory are regarded by many eminent geologists as having a fatal weakness because they are based on conditions supposed to exist beneath the earth's crust, but yet entirely unknown. The presence of a molten zone can neither be proved nor disproved until these conditions are better known. It is certain, however, that Sir William Thomson's conclusion as to the rigidity of the earth's crust is not in accordance with the facts; for the fact is that folding, fracture, displacement, and yielding in all directions are ubiquitous and constant phenomena in the history of the rocks.

The theory that earthquakes are due mainly to displacement resulting from contraction due to cooling is,

however, equally tenable on the supposition of a solid globe. Unequal contraction of such a globe cannot fail to produce displacement and fracture.

DECEMBER 15, 1886.—FIFTIETH REGULAR MEETING.

William G. Stevenson, M. D., chairman, presiding.
The following paper was read:

PRIMORDIAL ROCKS OF THE WAPPINGER VALLEY LIMESTONES AND ASSOCIATE STRATA.

BY PROF. WILLIAM B. DWIGHT.

In a paper read before the Section last spring (see page 130 of this vol.), a description was given of Potsdam strata discovered early in 1885 on the farm of Mr. A. K. Smiley near the driving-park.

The lack, at that time, of sufficiently extended paleontological evidence, left it very uncertain what proportion of the width of the western of the three parallel limestone belts might be primordial, and what portion might be occupied with the higher limestone strata of the vicinity. The only evidence on this point was the presence of a narrow strip of fossiliferous Trenton along the extreme eastern margin, at R. J. Kimlin's farm.

As the result of continued field-work, it is now in my power to present very material and important additions to our knowledge of the primordial rocks in Dutchess county.

I may describe first the points of information gained with reference to the above mentioned western belt lying south of the driving park. A ledge of Potsdam, containing a few specimens of *Lingulepis pinniformis*, has been found on R. J. Kimlin's farm. It is a few rods east of the road, and a little north of his barn. It is thus near the eastern margin of the belt, and only a

little west of the line of the Trenton ledge previously found on Kimlin's farm. The lithological character of the strata would indicate that much of the rock in this neighborhood is of the Potsdam horizon, though it is difficult to find in it fossils of any kind. A subsequent discovery made on April 28th, corroborates the evidence of the facts on Kimlin's farm, while it is far more interesting and instructive. The Spackenkill creek road leaves the south road (on Albany post road,) at school house No. 2, and leads easterly; it therefore crosses this limestone belt. At a point in this road a little less than a mile from the school house, a road crosses into it from the Varick farm-house. At the junction of these roads, and extending some distance along the Varick road, is a low ledge of dark, compact limestone, lying mostly beneath the surface of the ground, which has proved to be rich in Potsdam fossils. The best locality for these was about a hundred feet from the Spackenkill road, and almost in the middle of the Varick road. By making an excavation here, under permission of the roadmaster, I obtained quite a considerable supply of slabs and smaller specimens, in which glabellas and free cheeks of trilobites, in an excellent state of preservation, and other fossils, were crowded together. *Lingulepis pinniformis*, and the allied species found on the Smiley farm, are quite frequent here. The trilobites are all of two species not yet identified among the trilobites of the last-mentioned locality. These are respectively, *Ptychoparia saratogensis*, Walcott, and *Ptychoparia calcifera*, Walcott; previously discovered by Mr. C. D. Walcott in the Potsdam strata of Saratoga county; they were described by him (under the names *Bathyurus armatus*, and *Conocephalites calcifera*), in the *Thirty-Second Annual Report of the New York State Museum of Natural History*, but he has not yet published any figures of them. They may be readily dis-

tinguished from each other by the fact that *P. Saratogensis* has a glabella generally smaller than that of the other trilobite and with nearly parallel sides, a very narrow and simply convex front margin, very small eye-lobes, and no occipital spine; while the *P. Calcifera* has a glabella more conical in form, broadening posteriorly, a very broad front margin with a doubly-curved surface, very large and furrowed eye-lobes, and an occipital spine. It is instructive to note that this locality of Poughkeepsie Potsdam, and probably the Smiley strata also, are thus correlated with the Potsdam of Saratoga county.

In my former paper, the Potsdam was identified as continuous along the western margin of this limestone belt to the Hudson River. The new localities above described lie along a line which runs close to the eastern border of the belt. This suggests the possibility that the main width of the limestone here, of over a mile, may consist of folds of Potsdam strata. The lithological features are more in favor of this theory than against it. These Potsdam strata, along their eastern edge, appear, at one point at least, to be faulted against Trenton strata, *i. e.*, on Kimlin's farm.

The locality on the summit of the most eastern of these three limestone belts, south of the MacPherson (formerly Boardman) place, mentioned in my former paper as probably Potsdam, is such, without much doubt. Here also these primordial rocks are in close juxtaposition with highly fossiliferous Trenton strata, which lie immediately to the east, indicating a probable strike-fault.

Another very interesting locality of fossiliferous Potsdam has been developed some distance further to the northward. It is in the main belt of the Wappinger limestones, almost midway between Pleasant Valley and Salt Point. The branch road passing towards the

Wappinger creek, from the main valley road, near schoolhouse No. 12, a few rods southeast of the latter building, makes a sharp turn to the north as it crosses the summit of the limestone ridge, here quite low. Exactly at this turn, both in the road itself and in the fields of Mr. Paul Flagler's farm, on either side, the rock, consisting of calcareous shales, passing into compact arenaceous limestone, yields excellent Potsdam fossils. So far as examined, these consist of *Lingulepis pinniformis*, and an undescribed brachiopod.

No trilobites have as yet been found here. These primordial strata evidently continue for over half a mile to the northward, exposed in ledges along the higher parts of the ridge. They are then mostly concealed under drift, but they reappear in a narrow layer of shales, yielding *Lingulepis pinniformis*, lying at the eastern base of the bold precipice of Trenton and Calciferous (?) limestones at the southern end of Wallace's quarry. These potsdam layers are two or three rods west of the farmhouse occupied by Mr. Tabor at the point where the road suddenly turns east to cross the Wappinger creek. This fact adds a complication to the already difficult problem of the stratigraphy in the neighborhood of this most interesting quarry.

It is now my impression that there is a considerable breadth of outcrop of primordial strata between Salt Point and Pleasant Valley; at the same time it must be noted that there is abundant evidence by fossils of the presence of a considerable amount also of the higher limestones of this vicinity.

It remains now to note the discovery of primordial strata of yet lower horizon than the Potsdam. During the last week of October, and the first week of November, 1886, Mr. Charles D. Walcott, of the United States geological survey, and the writer, made several joint trips of observation to various points of interest in the

complicated stratigraphy of the western portion of Dutchess county. We spent the first and second days of October in a visit to Stissing mountain. Our chief object was to find paleontological evidence of the stratigraphical horizon of the quartzyte which immediately overlies the Archæan gneiss of the mountain. This quartzyte has been assigned to Potsdam by Prof. Mather (in the N. Y. state survey), Dana, and others, but on purely stratigraphical grounds, since fossils have never before been found in it. In June of 1884, Mr. S. W. Ford and the writer visited the locality between two trains. In the limited time at our disposal, we did scarcely more than to make ourselves somewhat acquainted with the topography and the more accessible points of observation. The only quartzose rocks which we examined, were those of a solid, very white, compact quartzyte forming conspicuous ledges on the lower slopes of the mountain at the south end, which is the only part which we visited.

On the occasion of the visit made by Mr. Walcott and myself, we examined much more carefully the same part of the mountain. Leaving the Stissing railway station, which is situated on shales of the Hudson river group, we followed the road running up the valley along the eastern flank of Mt. Stissing; the shales soon gave place to a compact bluish limestone, which in turn was followed by a red shale of lively color. Taking the first farm-road which, leading west, passed over the southern end of the mountain, we crossed another belt of limestone, which, at a point a little further up the main valley road, has been quarried and burnt for lime. After ascending some distance, we passed from the limestone to the quartzose strata which are in turn followed, on further ascent, by the underlying gneiss which forms the mass of the ridge.

The compact, white quartzyte in the bold terminal

ledges, as before, proved unfossiliferous; but as we struck away from the farm-road into the woods to the north, Mr. Walcott soon found loose fragments of less compact, ferruginious, decomposing, quartzose rock, completely filled with organic remains; among these could be detected glabellas and spines of *Olenellus*, with a species of brachiopod, and other fossils. In a few minutes we succeeded in finding this fossiliferous rock in place, showing, in great abundance, its fossils which had mostly been injured by the oxidation of iron, Mr. Walcott soon returned to the ledges of the limestone which immediately overlies the quartzyte, where it was exposed in the farm-road, and by discovering there opercula of *Hyolithellus micans*, thus proved that it belongs to the same horizon as the quartzose strata.

These fossils were also found in the little limestone quarry above mentioned.

Thus the question of the geological age of the quartzyte of Stissing mountain, as also that at the base of Fishkill mountain, (which we visited together immediately after leaving Stissing,) after years of uncertain speculation, has been solved; and its solution covers also the knowledge of the age of the immediately overlying limestone. The credit of these discoveries belongs chiefly to Mr. Walcott, whose long and intimate acquaintance with the pre-Potsdam strata, was of great service in the search.

The fossils discovered have been identified by Mr. Walcott, so far as the present imperfect specimens will permit, as follows:

From the quartzyte:—*Olenellus asaphoides*, or *Thomsoni;* probably the former; glabellas and cheek spines; brachiopods, probably *Triplesia*, and *Obolella*, species not determined. From the limestone immediately overlying the quartzyte:—*Hyolithellus micans*; opercula very well defined, rather abundant. *Obolella*, sp.?

These strata dip away at a very low angle (from 6° to 10°) from the axis of the mountain; but the dip of the adjoining strata increases rapidly with the distance from the mountain, until it reaches the usual angle of the Wappinger valley strata, that is, from 30° to 45°; with the increasing dip, it also acquires the predominant northeast and northwest strike.

The *Olenellus* quartzyte has an estimated thickness of about one hundred sixty feet, and the associated *Olenellus* limestone, one of about seventy-five feet.

About sixty feet of red shale overlie the *Olenellus* limestone, and over the shale there are about three hundred feet of limestone, which, in turn, is surrounded by the prevalent Hudson river shales of the valley. We found no fossils in red shale, nor in the higher limestones. In the absence of paleontological evidence, the more evident stratigraphy of Fishkill mountain establishes the presumption that the red shales, belong to the Hudson river group, and the upper limestone to a post-Potsdam age.

We examined the strata to the distance of about two miles from the south end of Mt. Stissing, in a southwesterly direction. We found them much faulted, so that the *Olenellus* quartzyte is brought again to the surface. It was evident that nothing short of a protracted and very careful examination would suffice to develop the true stratigraphic relations of that district.

The succession of the strata in the eastern part of Dutchess county, and doubtless of the entire county, is probably completely revealed by the acquisition of these facts concerning Stissing mountain. It is as follows, beginning with the lowest strata:

1. The Archæan gneiss of Stissing and Fishkill mountains, and of other elevations.

2. The *Olenellus* group, (Georgia group of Vermont,)

of quartzose and limestone rocks at the base of the above named mountains.

3. The Potsdam, (or upper Cambrian,) well exposed at Salt Point, and a little southeast of Poughkeepsie.

4. The Rochdale group, (Calciferous?) This group, with its unique set of numerous fossils, yet only very partially described, is the one which in previous papers I have called the Calciferous, because I considered it manifestly most closely related to what has been generally covered by that title. It is evident, however, that the proper limits of both of the terms, Calciferous and Chazy, (and of course that entirely vague expression "Quebec group") are undergoing severe review in the light of recent developments, and that many fossils heretofore assigned to one, may be ultimately found to belong to the other. I have decided, therefore, to designate these strata provisionally by the name of the locality where their fauna are most richly represented. It must not, however, be inferred that the strata so designated are the only ones found at Rochdale, for at least the Trenton, and the Hudson river groups are also well represented there. The rocks of the "Rochdale" group are apparently found everywhere in this limestone belt.

5. The Trenton limestone, found quite fossiliferous at Wallace's quarry, Salt Point, at Pleasant Valley, Rochdale and at Newburgh, and generally in the Wappinger limestone.

6. The Utica slate may be present in the county, though the fossils found along the banks of the Hudson river, and assigned to this group are thought by some experienced paleontologists to belong quite probably to the Hudson river group, on the ground of having been actually found in several places mingled with organic remains characteristic of the latter horizon.

7. The Hudson river shales prevalent in every part of Dutchess county.

This comprehensive view of the stratigraphy of this county, shows it to be manifestly a continuation of the strata of the Taconic and the adjoining series lying to the northward in New York and Vermont, and in the western portion of Massachusetts.

Taking these most recent paleontological developments with the facts of very much folding and faulting of the strata, it is also evident that the complexity of the local stratigraphy is very great. No stratigraphic chart of Dutchess county will be of much value which is not founded on field-work conducted with laborious detail.

JANUARY 12, 1887—FIFTY-FIRST REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding; forty members and guests present.

LeRoy C. Cooley, Ph.D., gave an interesting address entitled: "From Coal Tar to the Alizarine Dyes," in which he illustrated the progress made by chemistry in the analysis and synthesis of organic compounds.

No manuscript of this address has been prepared for publication.

Mr. James Winne was elected an active member.

FEBRUARY 9, 1887—FIFTY-SECOND REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding.

The following paper was read:

ON THE USE OF IODINE IN BLOWPIPING.

BY MR. CHARLES L. BRISTOL.

In 1866 Bunsen studied iodine films and their application to blowpiping and he is the first to define the results thus obtained. His method was to support the sample to be tested on asbestos threads and hold it in the upper oxidizing flame of the burner that bears his name. The volatilized oxides were condensed on the surface of

a porcelain dish (half-filled with water) held over the flame and this oxide film was then changed to an iodide film by exposure to hydriodic acid. In practice this was accomplished by setting the dish in the mouth of a salt-mouth bottle containing hydriodic acid and phosphorus acid obtained from the gradual deliquescence of phosphoric triiodide. Another method was to volatilize iodine held on asbestos threads and condense it on the film. The film becomes an iodide film in both cases. Bunsen prepared a table differentiating these films, but as the results are very similar to those described later, I omit them.

The next step was taken by Von Kobell, who published the use of potassium iodide as a means of distinguishing bismuth from lead, in 1871. He mixed the powdered sample with an equal bulk of a mixture of potassium iodide one part, powdered sulphur five parts and treated the mixture on a long coal with a moderate reducing flame. If the sample is lead, a rather volatile coat of yellow iodide of lead is formed at quite a distance from the assay. In samples containing little lead, the yellow coating may be masked by the white coat of potassium iodide, and if mercury, arsenic or antimony be present, they may either yield similar coats or may completely mask it. Bismuth under this treatment yields a beautiful red coat of bismuth iodide, at a great distance from the sample ;—*if the test is properly made*, says Cornwall. This may be masked ; either appearing similar to lead or covered by lead itself. To increase the delicacy of the test, especially in the presence of much lead, proceed as follows : The assay is heated over an alcohol flame in an open tube about 100 mm long by 80 mm in diameter with somewhat more than its bulk of a mixture of potassium iodide one part, sulphur five parts. If bismuth be present, its sublimate will appear as a dull brick red band or irregular streak about 8 mm

above the copious plumbic iodide. This is a test requiring much skill and good judgment.

These are the only tests employing iodine in blowpiping that I can find published prior to 1883, and I want to call your attention to two salient features in them.

Bunsen's films demanded a white surface rapidly chilled.

Von Kobell's films demanded a support that should withstand the heat of the blowpipe flame.

The next step was published in 1883 by Dr. E. Haanel, Professor of chemistry in Victoria college, Coburg, Province of Ontario, in the *Transactions of the Royal Society of Canada.* He was led, he says in his paper, to remove the difficulty experienced by students in distinguishing the oxide coatings of bismuth on charcoal from the similar one of lead by converting both these coatings into iodides. The method was to drop hydriodic acid on the coating in question and then direct the flame upon the charcoal just in front of the moistened spot. The heat of the flame volatilized the respective iodides which were again deposited on the charcoal at a greater distance, and assumed characteristic colors: bismuth, brown; lead, canary yellow. This was an improvement on Von Kobell's method since a suspected film could readily be differentiated. The drawback was the use of the unstable hydriodic acid.

In extending his observations, he soon came to find that charcoal was not a satisfactory support on which to utilize to the fullest extent the peculiar reactions of hydriodic acid, so he set out to find a substitute. It must be cheap and readily made. Its surface must be smooth and white, uninfluenced by the flame and readily chilled. The first two are Bunsen's conditions; the last two are Von Kobell's. Dr. Haanel's additional requirement was that it should absorb hydriodic acid. After much experimentation he produced tablets made of plaster of

Paris that combine all these conditions in an eminent degree. They are readily made by spreading a thickish paste of plaster over an oiled glass surface about one-eighth of an inch thick. While yet moist and before the plaster has fully set, the surface is cut with a knife guided by a ruler, so as to mark off tablets about four by one and three-quarters inches, though slightly smaller tablets may be used with as good results. When dry they easily separate and yield a glossy, white surface.

The hydriodic acid is prepared by passing sulphuretted hydrogen through water containing finely divided iodine in suspension. The resulting clear liquid is the resulting acid dissolved in water containing more or less hydrogen sulphide. This latter, however, is no hindrance; in fact it is an advantage, since it retards the decomposition of the unstable acid and in no way interferes with the reaction. The instability of the acid is practically of little importance, says Haanel, because free iodine is readily reconverted by passing hydrogen sulphide into the solution.

In manipulation, the assay is placed near one end of the tablet in a slight cavity, if necessary, and moistened with one or two drops of hydriodic acid, which is at once absorbed by the surrounding plaster. The assay is now gently treated with the oxidizing flame, and certain oxides, chlorides, bromides and sulphides are thus decomposed by the hot vapor of the acid rising from the tablet around the assay; and any volatile iodides are deposited upon the tablet at greater or less distance, according to the degree of volatility. These coatings are in some cases very striking and characteristic, not to say beautiful.[1]

At this point I must introduce another important improvement in the use of iodine, made by Messrs.

1 See Trans. Royal Soc. of Canada. 1883.

Wheeler and Luedeking, of Washington university, St. Louis, Mo., and published in the *Transactions of the Academy of Science*, of that city (Vol. IV, No. 4). While working on Dr. Haanel's method they found that the alcoholic tincture of iodine served as well in the case of all sulphides as hydriodic acid, and that the dry scales of iodine worked equally well with the sulphides. The next step was easy and natural, perhaps suggested by Von Kobell's experiment with potassium iodide and sulphur. It was to mix iodine and sulphur so as to convert all compounds to sulphides. This they accomplished by adding iodine to melted sulphur and pouring the liquid mass on cool glass. The resulting mass is dark iron gray with a tinge of brown, and consists of sulphur iodide, $S_2 I_2$, in an excess of sulphur. The proportions recommended by them are forty parts of iodine to sixty of sulphur. The cooled mass is broken up, powdered and kept in a stoppered bottle; when used it is mixed, slightly in excess, with the powdered sample and the mass is treated with the oxidizing flame. The superiority of the solid reagent offered by Messrs. Wheeler and Luedeking over the unstable hydriodic acid of Haanel must be at once apparent. It is more portable and is always ready for immediate use—a feature of no slight advantage in general practice.

It is impossible to give any adequate description of the results without the samples, though Haanel's paper as well as Wheeler and Luedeking's are beautifully illustrated in chromo lithography.

The adjectives used in the description of the films, taken from Haanel, are determined from the spectrum of the film in question. This can readily be done as follows: Support a small piece from the end of a plaster tablet in plane with a clean piece of glass, scraping the plaster down to the same thickness as the glass. Place the assay on the plaster and gently direct the

flame so as to cover the glass. After a few trials success will be easy, save in the cases of some of the more infusible substances. These films may then be used in the spectroscope or for projection. A comparison of Table No. 1 with Table No. 2 will indicate the prominent differences, and show the analogy between the iodides formed by the wet method and those by the dry.

Table No. 3 shows the differentiation of the coatings by the use of ammonium hydrate and ammonium sulphide. In differentiating similar coatings, ammonium hydrate and ammonium sulphide are used. They are most conveniently applied from a small wash-bottle containing the reagent, and having the mouth tube dipping under the liquid and the exit tube cut off close below the cork. Their action may be somewhat hastened by breathing. Iodine itself gives a fugitive brown coating that disappears rapidly, and must not be mistaken for a metallic coating. White coatings are rendered visible on the plaster of Paris tablet by coating it with lampblack. This furnishes a surface superior to charcoal and extends the value of the plaster tablet.

Table No. 4 shows an interesting and suggestive feature of these coatings and one worthy of thought. It is the relation of color to the atomic weight of the substances when they are arranged according to Mendelejeff's system.

TABLE NO. 1.

COLOR OF IODIDES FORMED IN THE WET METHOD.

(Roscoe and Schorlemmer.)

PbI_2—When a solution of a lead salt is mixed with a soluble iodide, a yellow precipitate is formed of PbI_2, which separates into yellow laminæ. On heating, this becomes reddish yellow, then light red, and lastly brownish black.

Tl I—Similar to Pb.

Cu_2 I_2—White crystalline powder: melts at red heat, solidifying to a brownish-white mass which yields a green powder.

Ag I—Light yellowish powder.

Hg_2 I_2—Greenish powders or yellowish crystals which, on heating, become red, then darker red.

Hg I_2—Scarlet crystalline powder, yellow on heating.

Hg I_6—(Mercury periodide) brown crystalline powder.

Sn I_4—Yellowish red.

Sb I_3—Brownish red.

Bi I_3—Greyish black.

Bi OI—Copper red crystalline mass.

TABLE NO. 2.

COLOR OF IODIDE COATS. (Haanel.)

ARSENIC—A reddish orange.

LEAD—A chrome yellow.

TIN—A brownish yellow.

SILVER—A bright yellow while hot, faint grayish yellow when cold ; is close to the assay.

ANTIMONY—An orange red.

MERCURY—Scarlet and yellow, the yellow changing completely to scarlet on standing.

SELENIUM—A reddish brown.

TELLURIUM—A purplish brown.

BISMUTH—A chocolate brown, fringed with red near the assay.

COBALT—A greenish brown edged with green ; the brown color is evanescent, changing into faint green, especially when breathed upon.

MOLYBDENUM—A deep ultramarine ; is close to the assay and is the permanent oxide, Mo_2 O_5.

WOLFRAM—A faint greenish blue; is a permanent oxide, $Wo_2 O_5$.

COPPER—White.

CADMIUM—White.

ZINC—White; is very volatile.

TABLE NO. 3.

DIFFERENTIATION OF IODIDE COATS.

Substance.	Am HO.	Am_2 S.	
Pb	Disappears slowly for a time.	to black.	
Sn	Disappears at once	whitish yellow.	
Ag	—	—	Film heated before flame, but not touched, changes to bright yellow. When touched changes to grey.
As	Disappears.	to lemon yellow.	
Sb	—	to orange.	
Hg	—	to greenish black: excess to black.	
Te	Disappears.		
Bi	To cherry red—excess gives transient yellow.	to black.	
Zn	Disappears. Only white coat that does.		
Cd	—	to orange.	

TABLE NO. 4.

Showing the Relation of Film Colors to Atomic Weights according to Mendelejeff's Periodic Law.

4th Period.			6th Period.			9th Period.		
Cu	63	White.	Ag	108	Bright yellow.	Au	199	———
Zn	65	White.	Cd	112	White.	Hg	200	Scarlet & yellow.
Ga	68	———	In	113	———	Tl	204	———
...	..	———	Sn	118	Brownish yellow	Pb	207	Chrome yellow.
As	75	Reddish orange.	Sb	122	Orange red.	Bi	210	Chocolate brown
Se	78	Reddish brown.	Te	125	Purplish Brown.	...	...	———
Br	80	———	I	127	Brown.	...	...	———

FEBRUARY 23, 1887—FIFTY-THIRD REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding; forty-five members and guests present.

The following paper was read:

BACTERIA.

BY MISS ISABEL MULFORD.

Judging from the literature which has lately arisen upon this subject, one readily infers that it is one of great and increasing interest, not only to those who are practically engaged in microscopical research, but to those who are interested in the great problems of life and its environment,—to those who are striving to learn more of the wonderful phenomena attending life, and death, and decay.

A few forms of bacteria were observed a long time ago. Leeuwenhoek in 1675, O. F. Müller in 1773, and Ehrenberg in 1838, described several forms, and made some attempt at classification, but until quite recently the notions concerning these lowly organisms were exceedingly vague and crude, and no attempt was made to control their action.

Though the study of bacteria presupposes microscopical work, since the largest cells cannot be seen except by the aid of a good microscope, many forms are

distinctly visible in the mass. Their chemical and physiological effects, too, are very apparent and were made use of in practical life long before the bacteria themselves had ever been thought of. Wine and vinegar, and beer and bread were made, and the risks attending their manufacture were known, but until the time of Pasteur, no one dreamed of bringing in the microscope as an accessory. He taught his countrymen to know with certainty when the ferments used were pure, and when they were invaded by diseased organisms.

Bacteria are developed in connection with organic matter whose vital energy is weakened or impaired. When exposed to the atmosphere the juice of fruits ferments. The clear liquid becomes cloudy or turbid, and deposits a sediment. Bright spots of color appear upon solids. A minute quantity of these substances placed under the microscope reveals myriads of minute organisms, the active agents in these changes.

When these and other fungous growths were seen developing in lifeless matter without any apparent cause, the theory of spontaneous generation arose. Under certain favoring conditions, imperfectly understood, life was supposed to assert itself and arise, like Phœnix, from the ashes. Many eminent scientists, from the time of Aristotle down, believed that the lowlier forms of life were able to spring spontaneously into existence. A few dared to dispute the theory, and some interesting discoveries took place, but its complete refutation was reserved to men of our day. Germs of these organisms were shown to exist in the air, the earth, the water. Some of the most interesting experiments known to science, were made by Pasteur, Tyndall and others, all serving to show that organic matter, free from these germs and cut off from contact with air, will resist decay for an indefinite period, and, even after the lapse

of years, will be found entirely free from microscopic life.

We have not the slightest ground for believing that any form of life exists which has not had its origin in some pre-existing organism of like nature.

These minute organisms lie on the border-land between the animal and vegetable kingdoms, but the great weight of authority now classes them with plant life, because their cells are coated with a membrane of cellulose and because they are able to assimilate nitrogen from inorganic compounds.

There is still great diversity of opinion as to whether they belong to the algæ or to the fungi. They are doubtless allied to the *Oscillatoria*, among the algæ, but as they do not develop chlorophyl, the tendency is to class them with the fungi.

With reference to their physiological effects, the bacteria are divided into three groups:

1. Chromogenes, producing pigments.
2. Zymogenes, producing fermentations and putrefactions.
3. Pathogenes, those forms observed in connection with disease.

The generic distinctions are based chiefly upon the form of the cells, which may be spherical, oblong, cylindrical, spiral, etc. Those bacteria whose cells are spherical or nearly so, belong to the genus *Micrococcus*, those whose cells are short, cylindrical rods, belong to the genus *Bacterium*, and those whose rods are united in long filaments producing endogenous spores, belong to the genus *Bacillus*.

Bacteria are the smallest living organisms of which we have any knowledge. Many are with difficulty detected even with our highest magnifying powers. A single cell constitutes an individual. Some forms occur singly, some are slightly united in chains or filaments, some

forms swarm in liquids, some develop a film or mycoderma, such as the "mother" of vinegar, and some develop a gelatinous mass or zooglœa, in which the separate cells are imbedded.

The movements of bacteria are thus described by M. Cohn:

"In certain conditions, they are excessively mobile; and when they swarm in a drop of water, they present an attractive spectacle, similar to that of a swarm of gnats, or an ant-hill. The bacteria advance, swimming, then retreat without turning about, or even describe circular lines. At one time they advance with the rapidity of an arrow, at another, they turn upon themselves like a top; sometimes they remain motionless for a long time, and then dart off like a flash. The long rod-bacteria twist their bodies in swimming, sometimes slowly, sometimes with address and agility, as if they tried to force for themselves a passage through obstacles. It is thus that a fish seeks its way through aquatic plants. They remain sometimes quiet, as if to repose an instant; suddenly the little rod commences to oscillate and then to swim briskly backwards to again throw itself forward some instants after. All of these movements are accompanied by a second movement analogous to that of a screw which moves in a nut. When the *Vibrios* in the shape of a gimlet turn rapidly round their axis, they produce a singular illusion; one would believe that they twisted like an eel, although they are extremely rigid."

The bacteria are multiplied by fission. A cell elongates, forms a partition wall in the centre, and the parts finally separate and thus make two distinct cells or plants. Under favoring conditions of temperature and moisture, together with abundance of nutriment, this multiplication is exceedingly rapid. Cohn has calculated that a bacterium which divides into two in the space

of an hour, in four at the end of a second hour, in eight in three hours, etc., will in twenty-four hours amount to more than sixteen and one-half millions. *Bacterium termo*, a common species, whose cells measure only one-twenty-thousandth of an inch in diameter multiply so rapidly that, at the end of a second day, they will fill a pint measure, and the same conditions being continued, the bacteria issuing from a single germ would fill the ocean in five days.

Of course it is impossible that these conditions shall ever arise. Owing to the nice adjustment of the powers of nature, no one form of life is able so completely to overpower the others,—still, bacteria are more widely dispersed and more numerous than all other living things put together. They resist the greatest extremes of heat and cold, and are known to live for years and then germinate with unimpaired vigor.

So much is said and written nowadays of the germ theory of disease that people associate bacteria only with thoughts of dirt, of disease, of death and decay, yet, in the great economy of nature, they have a work to perform—and that a very important one. In the organic world, change is the constant law. Our physicians tell us that our bodies are undergoing ceaseless changes. With each successive activity of mind and body a certain amount of material is used up. Everywhere in nature do we see these changes going on. Life is progressive and ever seeks new material upon which to work. Nature repeats herself in ever-recurring cycles. While life remains in a body, the vital principle has control of these changes ; but it has been shown that organic matter, once dispossessed of the principle of life, cannot again enter into the general circulation, until it has been disintegrated, and returned to the organic world. This work is relegated to the lower fungi. Their germs are about us everywhere ; in the earth, in the

water, in the air we breathe. They find their appropriate soil, and stand ready to seize upon any and every form of waste or lifeless organic matter. The yeast plant is nourished by the saccharine material it gets from the flour, and it gives off carbonic dioxide and alcohol. The vinegar plant feeds upon the wine and gives off acetic acid and water. Thus these lowly organisms reduce the higher complex substances to simpler ones, and in a body undergoing decomposition one form succeeds another until at last the elemental state is reached, and the constituent parts of the body are ready to be used again by the green plants, aptly called the architects of the universe. They and they only have the power of taking inorganic materials from the earth, air, and water, and converting them into living organic tissues.

The bacteria have done a large part of the work of removing the old trunks of trees and other débris of dead plants and animals which otherwise would have accumulated through the long ages. It is claimed that without bacteria we should have no fertile soil, no rich harvests. They render it possible to use the soil over and over again, and thus secure to us a constant succession of useful plants.

Bacteria cause the waters of close reservoirs to work and thus render the water sweet and clear for our use. Bacteria rot flax and release the fibre for the manufactory. They cause diseases of noxious insects and thus reduce their numbers. Either bacteria or the yeast plants, are the active agents in the various forms of fermentation, and so supply us with bread, beer, wine, and vinegar.

Against these good offices we must offset the fact that certain forms of bacteria are always found as an accompaniment of some of the most virulent diseases of mankind and the domestic animals.

The old fairy tales are true, after all. Here we have good genii, invisibly yoked to our service, making life easy and pleasant, and in many ways ministering to our comfort and happiness ; or, if we refuse to understand and obey good and wholesome law, we find in them the greatest scourges which afflict mankind. Like fire, they make good servants, but bad masters.

The extraordinary success which has crowned the labors of Pasteur, the remarkable work of Koch, Cohn, and others, have given a great impulse to the study of the pathogenic forms. It is impossible to predict the results. Much is hoped for, and it may be that this will cause the study of medicine to be placed upon a surer and more scientific basis than has heretofore been possible.

NOTES ON A FEW FORMS OF BACTERIA FOUND AT VASSAR COLLEGE.

During a few months of last year, I became much interested in making a series of experiments with a view to seeing what forms of bacteria would naturally develop in this locality, when culture media were placed under proper conditions. My special interest in the subject was first awakened while studying at the botanical gardens in Cambridge, during the summer of 1884. Dr. William Trelease had just written his paper upon bacteria. At his suggestion and under his direction, I then undertook some cultures and learned to recognize a few common forms. The present work has been rendered possible through his kindly encouragement and valuable assistance. I have adopted the plans proposed by him, and have succeeded in finding nearly all the species mentioned in his paper, together with a few other forms, which have puzzled me not a little.

The culture media employed were boiled or baked potatoes, the whites of hard-boiled eggs, chicken bouillon, infusions of vegetables, milk, etc. The work with potatoes was most satisfactory. These were cut in halves or slices. Their cut surfaces were then touched to a dusty surface or left for a time exposed to the air in order to inoculate them with possible germs. The potatoes were then placed upon plates of glass, under inverted tumblers. Several interesting forms quickly appeared, and at varying intervals of incubation, these were followed by others. The first cultures were of course badly mixed. It was not unusual to find several forms of bacteria contending for possession of the same slice, but by careful transfers, the species were separated, and a study of the development of individual forms was rendered possible. I soon became embarrassed with the richness of the field, so that I finally restricted my attention almost entirely to the chromogenic or colored forms. These I considered as distinct species, so long as their microscopic and macroscopic characters were constantly reproduced through successive cultivations. According to Cohn, the pigmentary bacteria are veritable species; for, first—their pigments offer the greatest diversity in chemical action and by spectroscopic analysis; second—each species, cultivated in the most diverse media, produces always the same coloring matter.

It seems to be evident, that there are a number of species indigenous to this country, as many forms cannot be identified with any described by the best German and English authorities; still, it is quite possible that further investigation will develop relationships which are not now apparent, and at present the tendency is to reduce the number of species as much as possible. I ought to say that I took special precautions to insure cleanliness and freedom from outside germs in my cul-

tures. Before making fresh cultures, glasses were carefully washed in hot water, and treated with a solution of corrosive sublimate. My knife and needle point were brought to a red heat, before cutting a potato or making an inoculation. Although this method does not always insure as pure cultures as the sterilized test-tubes plugged with cotton-wool, used in bacteriological laboratories, it is a tolerably certain one, and it has the advantage of showing how one form may follow and overpower another.

While engaged in this work, I took daily notes of the color, form, and texture of the zoogloeæ as well as of their microscopic characters. Slides were frequently prepared. In these, the bacteria were stained with aniline violet and mounted in benzol balsam. The microscopical work was done with a Zeiss one-eighteenth homogeneous immersion objective, and an Abbé condenser.

The following is simply a *résumé* of my observations :

I have not ventured to give names to the forms for which I find no adequate description, but for the present will designate them as species a, b, c, etc.

Micrococcus candidus, Cohn. I first met with this species in my Cambridge cultures, and it appeared quite frequently during my recent work. Its first appearance was in very minute milky dots which in a short time became confluent in milk-white mammillated masses. These afterward became dry, dull, and chalky. Occasionally a growth was seen coming through a small break in the skin of the potato. This was very dry, light, and powdery, and presented an appearance much like the efflorescence of a salt. The growth of the zoogloea is never very rapid, but it is persistent and seldom yields its place to other forms, even though their growth may be much more luxuriant. The cells are readily stained, are quite large, and show interest-

ing forms of grouping. They are spherical and measure .7–.9μ in diameter.

Micrococcus, species *d*. A slice of potato rubbed on the wall of a side room in the barn, developed in a few days a small patch of pink color. At first this was badly mixed with mucors and other weeds, but after a few inoculations these were excluded and the bright pink zoogloea succeeded in establishing itself as an independent growth of great vitality. This species was under my observation for over a month and during that time, many cultures were made, all showing the characters as described below.

The zoogloea shows a decided growth, visible to the naked eye within twenty-four hours after inoculation. It forms little conical masses along the line touched by the needle in inoculation. These are quickly confluent, and the growth soon spreads over the entire cut surface of the potato, and then extends downwards through the substance until the whole mass is deeply colored. If the lower side of the potato is cut, a thick growth appears there also, but the zoogloea does not penetrate the skin. The color is at first a beautiful tint of pink, but in two or three days the exterior portions become duller while the color is intensified below. The zoogloea has a semi-solid, waxy consistency. In growth, a slight fermentation is apparent, and the surface is raised in places, forming a pellicle over the bubbles of gas beneath, thus giving a blistered appearance to the potato. The raised part is quite thick, and never transparent or membranous. A bright orange or amber-colored zoogloea was occasionally seen associated with this form. As the *M. prodigiosus* is described as varying in color, I at first thought I had perhaps encountered that species, but I never saw one color develop from the other. If I transferred the orange, it developed an orange zoogloea, if the pink, the resulting growth was pink. As before

stated, the growth of the pink zooglæa was very rapid and very luxuriant ; the orange-colored, on the contrary, developed slowly and never spread over a large surface. Its cells, too, were smaller, so that I now regard the two forms as distinct species. The orange-colored form may possibly be the *M. prodigiosus*, but the pink certainly is not. It does not develop the form compared to fish spawn which is described as belonging to Cohn's species, and it does penetrate the substance of the potatoe, while the *M. prodigiosus* is a surface growth.

The cells are spherical, or more frequently oval, and sometimes considerably elongated. The shorter diameter is about the same as in *M. prodigiosus*, the larger varies from .7–1.5 μ.

Micrococcus prodigiosus, Cohn. One of my cultures of *Bacterium chlorinum* was attacked by a bright blood-red or carmine-colored zooglœa, which for a long time puzzled me greatly, as in connection with it I found cells of various descriptions. Micrococcoid cells were mingled with those of bacteria and bacilli in hopeless confusion. I took the utmost precaution to obtain pure cultures, but never succeeded entirely. The rod-like cells were evidently impurities. This red growth was sometimes covered with a wrinkled film, and sometimes presented a smooth, shining surface.

I have since made several cultures of *M. prodigiosus* from material kindly given by Prof. Farlow and others. The size and form of these cells, as well as the color and texture of the zooglœa when compared with my red cultures, were so evidently the same that I think there is no doubt that this red growth belonged to the *Micrococcus prodigiosus*.

At one time the cylindrical cells in my cultures were beset on all sides by little spherical granules, which I thought perhaps belonged to a parasitic *Micrococcus*,

but they were so extremely minute that I do not feel justified in making a positive statement.

Micrococcus aurantiacus, Cohn ; *Bacteridium aurantiacum*, Schröter. This species developed spontaneously upon the white of a hard-boiled egg placed under an inverted tumbler after a few moments exposure to air. It was first observed in about twenty-four hours afterward in the form of minute yellow dots. These grew to the size of a small pin-head, and multiplied in number until they became nearly confluent over a surface of about a square centimeter. The color was a bright, golden yellow, soluble in water.

Microscopic investigation showed these zoogloœ to consist of oval cells 1.5μ in their longer diameter.

Orange-colored zoogloœ having micrococcoid cells were also observed upon potatoes, but as cultures from these sometimes developed elongated, sometimes micrococcoid-oval cells and usually a mixture of both forms, I am in doubt as to whether my cultures were pure, or whether one form developed from the other.

Micrococcus luteus, Cohn ; *Bacteridium luteum*, Schröter. A potato whose cut surface was touched to the sink of the mineralogical laboratory January 16, quickly developed luxuriant growths of *Bacterium candidum* and *B. luteum* each occupying about equal halves of the potato. This culture I kept a long time to see whether either species would overpower the other. *B. candidum* at last spread over the outside of the potato, and penetrated tender or broken places in the skin.

B. luteum maintained its early position with reference to the other species mentioned but was not able to exclude other growths.

During the second week a small, smooth, reddish-brown spot appeared in its midst, which I at first imagined was an indication that this species as well as *B.*

candidum would grow deliquescent with age. Subsequent culture, however, showed the spot to consist of a mixture of various species, requiring different terms of incubation.

A culture made from this on March 12, developed a luxuriant, bright yellow, semi-fluid growth consisting of broadly oval cells in a gelatinous substratum. This soon spread over the surface of the potato and gradually assumed greater consistency until it was like soft wax.

This form was under my observation about a month, yet never in any culture developed the characters of either *B. aurantiacum* or *B. luteum*. Though the cells are ellipsoidal, rather than spherical, they are not at all slender, and as fresh cultures developed the same cells, I cannot believe them to be the gonidial stage belonging to *B. luteum*.

Bacterium candidum, Trelease. This species appeared very frequently so that it was difficult to exclude it from other cultures. Even when glasses were treated with corrosive sublimate, and needle point and knife were scrupulously fired before making an inoculation, it would develop and quickly overrun the rarer forms.

It is an interesting species on account of the various phases in the development of its external characters. In twenty-four hours after its inoculation, it forms a white, glistening, opalescent growth upon the surface of the culture medium. In the next stage, this surface is dulled, and a delicate white film is rapidly formed. This by degrees becomes slightly roughened until the surface seems sprinkled with a fine, snow-white powder, and a hand-lens shows that this appearance is due to a fine lobation of the surface. Now the zoogloœa is thickened and begins to wrinkle in characteristic folds, while the color deepens to a creamy color and finally to a brown.

At first the zoogloœa has scarcely more coherence than water, but in a short time it becomes so viscid that it may be drawn out in delicate threads more than half a metre in length. Prof. Trelease says that it makes a very adhesive paste.

These changes follow one another in rapid succession. In about two weeks from its first appearance, the folds of the surface disappear, and the mass becomes somewhat deliquescent and glistening; not infrequently it flows in a thick stream over the sides of the culture.

Under the microscope, this gelatinous mass is seen to be filled with rod-like cells, which are sometimes isolated, sometimes united in short filaments. A delicate cell-wall has been demonstrated. These cells have the power of motion when placed in water, but flagella have not been seen. The measurements are 1.6–7μ by .6–.8μ.

Bacterium aurantiacum, Trelease. In my cultures of last year, *B. aurantiacum* was one of the first species to attract my attention. At first appearing as a light yellow film, the zoogloœa gradually increases in thickness and consistence until it has a smooth, wavy appearance and a bright, orange color. This color is maintained for some time, but the old zoogloœæ change to a dull, dark brown. The cells are single or connected in chains. They have a rapid motion. The old cells often have a condensation of protoplasm at either end. In size they are considerably smaller than those of *B. candidum*; they measure 1–2μ by .4–.5μ. The cells are not readily stained by the ordinary violet fluid.

Bacterium luteum, Trelease. This also is a very common and a very persistent species. Its first appearance is quite similar to that of *B. aurantiacum*. The zoogloœa is semi-fluid, and though at the very beginning of growth it is nearly transparent, it soon shows a light yellow coloration.

Unlike *B. aurantiacum*, the surface is soon wrinkled and covered with a fine network of ridges. The color then rapidly changes, first to a light then to a dark brown with a reddish tinge; the latter color is persistent. The cells are .35–.5μ by .8–3μ. They may be single or in filaments of two to several. In the old zooglœa, the cells are much shorter—almost spherical.

Bacterium syncyanum, Schröter; *Vibrio syncyanus*, Ehrt. Some milk exposed to the air for several days, developed a luxuriant growth from one to two millimeters thick, of a deep blue color. The zooglœa was found to consist of slender bacteria which presumably belong to the above species, but for want of adequate descriptions, it could not be identified with certainty.

A blue form having similar cells, developed on the under side of a potato touched to the wall in a barn. Little groups of minute dots multiplied for three days with considerable rapidity, but growths of other species soon gained the ascendency and every attempt at culture was unsuccessful. As a consequence the species was soon lost. I have very little hesitation in referring the growth to the *B. syncyanum*.

Bacterium violaceum, Bergonzini. Glairy growths of violet zooglœa appeared at three different times, but always in connection with *B. luteum* or other species whose growth is rapid. I never succeeded in getting an absolutely pure culture, but at one time I had the species growing quite luxuriantly for about two weeks. It always developed on the under side of a culture, and I found it grew much better if covered with a thick cup which completely excluded the light.

While working in the botanic garden in Cambridge, I was given material for a culture of this species by Prof. Trelease, who received it from Prof. Farlow, in

whose laboratory it occurred spontaneously. My efforts at culture, were, however, unsuccessful.

The cells are very small, slender rods, measuring .3–.4μ by .6–1.6μ.

Bacterium incarnatum, Trelease. This is a rather common species. While maintaining the same microscopic characters, it shows considerable variation in color, It may be seen in all shades from a pale and dull flesh color to a deep, reddish brown, the cultures assuming a darker color as they grow old. The texture is smooth and waxy like that of *B. aurantiacum*. The cells are about .4μ in diameter, and from 1–2μ in length.

Bacterium chlorinum, Trelease; *Micrococcus chlorinus*, Cohn. It was with some difficulty that I obtained a pure culture of *B. chlorinum*. Though it has a good deal of vitality, it readily yields its place to other forms, even after it has made considerable growth in a clear field.

It is of a greenish-yellow or sometimes of a deep yellowish-green color, and when well established will cover the potato with a smooth growth of waxy consistence. The cells are rather short and measure .5μ by .8–1.6μ.

At one time a slice cut from the middle part of a potato was inoculated with this form. For a week or two I thought the culture decidedly pure, but it was afterwards overspread with a growth of *M. prodigiosus*, so that scarcely a trace of *chlorinum* was seen above or below. A month later, green zoogloeæ appeared in a few little round dots. The color was much brighter and more intense than the original *chlorinum*. The zoogloeæ contained multitudes of spherical cells mingled with a few rods. I felt inclined to think that this was *B. chlorinum* in a new stage of development; either a spore-bearing state, or one in which the cells became short and spherical from lack of sufficient nourishment. Prof. Trelease, however, whom I consulted, thought I

had found the *Micrococcus chlorinus* which Cohn observed on hard-boiled eggs.

Cultures made from these green dots, developed green zoogloeæ with rod-like cells, which tended to confirm my theory. They were slightly larger than the original *B. chlorinum* cells. With these were mingled a few spherical cells. The new zoogloea had a deep-green color with a reddish tinge, as might have been expected.

Upon one occasion I was kindly supplied by Prof. Dwight with a peculiar preparation of milk called youghwort, and much esteemed in Turkey. A culture of this developed several diverse forms of bacteria which were so badly mixed that my efforts at isolation were unsuccessful. The potatoes became eventually covered with a tough formation of a dull salmon color. This deserves more extended study than I was able to give.

Bacillus, species *a*. A bacillus producing a white zoogloea seems to be rather common here. It was observed in cultures from the main building, from the natural history laboratory, and from the barn.

As first seen this usually presented a grayish white coloration, with sometimes a slight greenish tinge. Many cultures were milk-white for a week or two, then became creamy, and finally brownish in color. The zoogloea varies in consistency from that of cream or soft lard to a nearly solid state, with a dry, roughish surface ; in general, it is waxy, and spreads smoothly over the cut surface of the potato.

In some cultures the cells agreed in size and form with those of *Bacterium candidum*, but as the zoogloea is never glairy, ropy, powdery, or wrinkled, it cannot by any possibility be referred to that species. More commonly the cells are much swollen and vacuolate. These occur in chains of several articles, or singly. Associated with these I frequently found little oval bodies which I

assume to be spores. The walls are readily stained with aniline violet but the centre remains colorless. In a few instances, these cells were seen to germinate. When deprived of proper nourishment the species seems to have a resting state. Hard, dry, white scales are formed. These transferred to a freshly boiled potato grow with marvelous rapidity. Long filaments are formed so full of spores as to resemble strings of delicate transparent beads. A very common measure of cells is 1μ by 1.5–2μ.

Bacillus, species *b*. A culture of *Bacterium incarnatum*, which behaved rather strangely, was kept under observation for several weeks. The zoogłœa had a brighter pink tint than most cultures of that species, and it was subjected to a very obvious but slow fermentation, which filled it with bubbles of gas, and gave off a strong odor of mulled corn. Microscopical examination revealed nothing unusual in its cells. At one side of this culture was a dirty white mass with a botryoidal surface, as seen with a Coddington lens. This was so close and tenacious in texture that I found it difficult to remove a part with my needle point for examination. In this tough, intercellular substance multitudes of bacilli were imbedded. A fresh potato inoculated with this material was soon filled with a most luxuriant growth. The whole substance of the potato was in a few days changed to a fermenting, transparent mass of semi-fluid consistency, and the room was filled with the same strong odor as before.

On the second day, cells were arranged in long filaments, each article of which showed a partition at its center, marking the line of fission. On the third day the fission was complete, and chains of spherical cells, like rows of conidial spores in miniature, were seen. With these were mingled a few long rods. In a short time multitudes of oval spores were freed from these

filaments. A week later, this yeasty liquid was changed to a semi-solid mass, much like pure gelatine in consistence and color. Under the microscope the same bacilli were seen, many as isolated cells, some growing from spores, and some in turgid vacuolate filaments. Upon the same potato there afterwards appeared a smooth white growth, resembling somewhat, in its external appearance, the zooglœa formed by the white bacillus previously described as species *a*. In making slides from this, a finely granular or gritty character was noticed when a minute quantity of the material was pressed between two cover glasses. These slides showed the microscopical characters as before described. Many cells measured .7μ by 2–3μ. Many others in rapidly growing filaments were much larger.

Bacillus, species *c*. A very old culture of February 5, from the main building showed a curious mixture of colors. It was an interesting study to watch the struggle for existence going on among the different forms as they successively appeared and disappeared. On March 15, a brilliant line of red, one centimeter long, was observed. A fresh potato was carefully inoculated with a very minute quantity of this zooglœa, and on the second day afterward, the cut surface was entirely covered with the bright red growth. Through many transfers, as well as in the cultures of the date mentioned, this species afterward maintained its distinct character. The zooglœa was at first fluid and glistening, but an exceedingly fine film rapidly developed over the surface. The color varies from orange to vermilion or indian red. At this stage the growth is in a very thin layer and the finely wrinkled film adheres closely to the potato, but after a few days the zooglœa becomes coarser and thicker, and the color of its outer portions changes to a dull brown. Underneath, however, it is still as brilliant as at first. It penetrates every crack

and fissure in the potato, but does not extend through the substance.

The cells were at first so nearly the size of *Bacterium incarnatum* as to suggest that species, though the color as certainly suggested *Bacillus ruber*. Later slides show somewhat larger cells,— some short and thick, others long and slender. Spore-bearing filaments are also seen. The walls of the little oval cells took the staining fluid well, but the interior remained colorless. The cells measured about .5-.7 by 1-3μ. in the earlier cultures.

Bacillus ruber (?). Among my earliest cultures, I obtained a pink zoogloœa with cells corresponding in size with those of the species mentioned, but the color was so far removed from that described by Frank, that I very doubtfully mention it here, particularly as I was unable to keep the zoogloœa more than four or five days.

Dr. Stevenson, in behalf of the Section, gave cordial thanks to Miss Mulford for her interesting and instructive paper, the subject of which was still further discussed by Messrs. Dwight, Cooley and Stevenson.

MARCH 9, 1887—FIFTY-FOURTH REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding; thirty-five members and guests present.

The following paper was read:

NEW STARS IN ANDROMEDA AND ORION.

BY MISS MARY W. WHITNEY.

I propose this evening to give a few facts about the new stars which have appeared in the heavens within the last two years, the *Nova Andromedæ* and the *Nova Orionis*. As the interest in a new star turns quite as much upon the light it may cast upon the nature and classification of earlier phenomena of like character as

upon its own peculiarities, I will preface my description by a brief account of some of the new stars which have been recorded in astronomical annals. For the time being astronomy adopts a division into two sections of those heavenly bodies which rise and fall in brilliancy. Those stars which burst into view, suddenly or gradually lose light, and then disappear, or if they do not wholly disappear from telescopic sight, become exceedingly faint bodies which show no further indication of change,—such are called "temporary stars." Those which vary in magnitude, whether regularly or irregularly, and whether through longer or shorter periods of time, are called variables. It at once suggests itself that temporary stars may be variables whose periods are of such enormous durations and whose changes are of such a spasmodic character that the centuries covered by astronomical records are not numerous enough to provide data for their detection. There is a difference between the two classes, however, to which I shall return later, *i.e.*, a difference in their quality and light as thus far revealed by spectrum analysis, and this divergence affords a better ground of separation than the definition given above.

The remarkable temporaries of earlier centuries are well known to readers of astronomical books ; that discovered by Hipparchus in 140 A. D., that by Tycho Brahe in 1572, and that by Kepler in 1604 holding prominent places, because of their superior brilliancy.

But the temporaries of 1866 and 1876 are of especial interest and importance, not because of transcendent brightness, but because of the great care with which they were observed, and pre-eminently, because they were the first new stars to which spectrum analysis was applied. On May 12, 1866, a Mr. Birmingham, in Ireland, saw a star of the second magnitude in the northern crown, which he recognized as a new appear-

ance in the heavens. Later it was learned that a careful observer, Schmidt, at Athens, had been scanning the region about Corona a few hours before Mr. Birmingham's discovery, and had seen no star in this position. On May 16, the star had fallen below the third magnitude, and about ten days after its first appearance, it became invisible to the naked eye, that is, it fell from the second to the sixth magnitude in ten days. It is now a faint telescopic star of the tenth magnitude, and as such it existed in the catalogue of Argelander before its sudden outburst.

In November 1876, Dr. Schmidt, of Athens, observed for the first time a star of about the third magnitude in the constellation Cygnus. No star is recorded by Argelander in the position of this temporary. Again, as in the case of the *Nova Coronæ* (called also *T. Coronæ*), the star decreased in luminosity from the date of its discovery, but far more slowly. It had fallen to the seventh magnitude by January of the following year. It has now passed beyond the ken of even powerful telescopes, and is perhaps about the fourteenth magnitude.

Because the light of these new stars died away so rapidly, it might be inferred that the conflagration producing the outburst was not, comparatively speaking, extensive, and therefore that the bodies must be among those stars which lie near to the earth. Dr. Ball, astronomer royal of Dublin, investigated the parallax of the *Nova Cygni*, but he could detect no large stellar parallax. Therefore it cannot be one of the nearer stars.

One other temporary I will mention, not so celebrated as those I have called your attention to, but bearing more closely upon the *Nova Andromedæ*. In May, 1860, Auwers discovered a new star of the seventh magnitude in the constellation Scorpio. The star lay in the midst of the nebula 80 Messier, which is a resolvable nebula, *i.e.*, a remote star cluster. By the middle of June

the star had fallen to the tenth magnitude. It has now entirely disappeared.

If we compare recent astronomical catalogues with early records, there would appear to exist many cases of disappearance of stars. In the majority of such cases, however, it is safe to assume that the want of accuracy in recording positions among early observers will explain the apparent vanishing of the star. But some such disappearances are undoubtedly authentic. For instance, there are a few stars recorded by so recent and careful an observer as Sir William Herschel, which are no longer found in the given places.

Variable stars are divided into various classes. Prof. Pickering gives five, placing temporaries in the first. The second class includes long period variables, whose changes run through several months. A third, short period variables, the duration of change not exceeding a few days ; a fourth, stars undergoing slight changes so irregular that no period can be suggested, such as *Alpha Orionis.* Into the fifth division fall the few stars of the Algol type (the eighth quite recently discovered) whose variations are remarkable, and yet of such regularity that the periods can be determined within a few seconds of time. The Algol class probably bears no relation to our subject of new stars.

Ceti, called *Mira Ceti*, is one of the most interesting variables of long period. At its maximum, it has attained to the third and even to the second magnitude and at its minimum it passes beyond visibility to the eighth or ninth magnitude. Its period is about eleven months, though quite irregular in its duration. About forty days after it becomes visible it attains its maximum and about sixty days following maximum it again becomes invisible ; that is, longer in losing light than gaining it. At its maximum in 1886 it reached only the 4.9 magnitude. This is almost the lowest recorded,

none standing lower except that of November, 1868, when it was rated as of the fifth magnitude.

I will allude to one other variable only, which is of interest both because of its singular character, and because it lies in a nebula. This is in Argus, too far south for our observation. This star has reached a brilliancy nearly equal to that of Sirius. This occurred in 1843. In 1867 it had fallen to the limits of visibility and soon after passed beyond it. It has not since that date begun again its ascent. The star lies in the central or more condensed portion of an extensive and irregular nebula, known as the "key hole nebula."

On August 31, 1885 the astronomical world was surprised by the announcement from Dr. Hartwig, of Dorpat, that the night before a new star had been seen by him near the nucleus of the great Andromeda nebula. Hartwig had suspected a change in the nebula as early as August 20, but unfavorable weather and defects in his instruments had prevented an earlier confirmation. He estimated the magnitude as about the seventh. After his announcement various other observers claimed to have seen the star at a still earlier date. The detection of the new body as early as August 19, by Mr. Ward in Ireland is generally acknowledged; whereas an observer in Florence, Mr. Temple, who knows the nebula well, maintains that it was not to be seen on August 15. The date of the outburst, therefore, can be placed between the fifteenth and twentieth of August, 1885.

The nebula in Andromeda in the midst of which the star lay is one of the oldest known in astronomy, since it is visible to the naked eye. As early as 1614 Marius gave that description of it so frequently repeated, that "it looks like a candle light seen through horn." It is elliptical in form. No telescope has been able to resolve it. Telescopes of high power show above a thousand stars within its area; also two dark rifts or spaces.

Since the application of spectrum analysis to the heavens, this nebula has become of especial interest, because it belongs to the class of those which, though irresolvable by the largest telescopes yet constructed, still give a spectrum which indicates that they must be wholly or largely non-gaseous.

The new star lay close by the nucleus, removed from it only 1.^{s}4 in right ascension and 4″ in declination. A small, eleventh, magnitude star preceded it by about seven seconds. It fell in brightness from the date of its discovery. Prof. Hall observed it with the twenty-six-inch Washington telescope early in September. He called the star red, and estimed its magnitude 7.5. His observations were continued till the star had declined to the sixteenth magnitude, *i.e.*, to about the limit of visibility in the twenty-six-inch glass. The star disappeared in February. Prof. Hall thought it grew less ruddy as it grew fainter. He also calls attention to the fact that ancient astronomers, who knew the nebula only through naked eye observation, held that it was variable both in light and form. Perhaps similar outbursts in earlier ages may account for this belief.

The star was sharply defined, its color red or reddish orange. It contrasted finely in this regard with the greenish white of the nebula. This light background of the nebula gave the star the appearance of a body observed during twilight. About September 17, the Potsdam observer noted a break in the falling off of light. Several additional measurements were taken to insure a correct estimate, and the result remained unchanged. Perhaps this indicated a slight renewal of the outburst.

The observations to be made upon a new star pertain to its color, its variability and its spectrum. I made a series of observations upon the *Nova Andromedæ* running from September 28 to October 14, with a three-inch

glass. After the latter date I lost sight of the star in the encroaching moonlight, and when the moon had passed, the star was too faint to be any longer followed in that glass. Perhaps it will not be out of place to give briefly the method for determining magnitude, which I used. It is very simple, suggested first by Argelander, and, as I use it, slightly modified by Prof. Pickering. First we must identify the star if it is not large enough to be recognized by the naked eye. For this purpose, by the aid of a good star catalogue, which gives both position and magnitude, we plot the stars whose right ascensions and declinations show them to be in the vicinity of the new body. Such, in the infinite variety of position and size presented by the heavenly bodies, that the picture formed by our grouping of neighboring stars is easily recognized as we sweep the telescope slowly over that portion of the skies which contains the sought for star. And if in that group there is found one not included in our catalogue picture, the temporary is identified. This process of identification was uncalled for in the case of the *Nova Andromedæ*, because of its singular position in the renowned nebula. But I needed to resort to it to identify the *Nova Orionis*. After the star is secured, certain other stars are selected, some brighter and some fainter than the star itself, and their magnitude determined from some trustworthy catalogue. Argelander's *Durchmusterumg*, a celebrated catalogue prepared at Bonn, is often used. It contains stars to the 9.5 magnitude. The new star is separately compared with a star slightly above it in magnitude, and with another slightly below it, and its relative position between the two estimated. Thus if we determine it to lie half-way between the two we make our record a 5 b, a and b representing our comparison stars. A similar comparison is made with other pairs of stars, the more the better, to secure accurate results. This very simple

method has been found to yield satisfactory determinations even when bearing comparison with the more exact instrumental methods, such as by wedge photometer and meridian photometer. During the period of my observation the Andromeda star fell with considerable regularity from the 8.4 to the 9.6 magnitude. On comparing my estimates with those published in the *Astronomical Nachrichten*, I found I had placed the magnitude somewhat larger throughout. This arose, doubtless, from my fear lest I should underrate it on account of its nebulous surrounding. It was noticed at Dum Echt that low powers showed the star relatively brighter than the large telescope, probably because low powers did not separate the star altogether from the surrounding nebulosity.

A few observers believed that they perceived a noticeable diminution of the brightness of the nebula during the maximum of the Nova, among them Hartwig. The majority of astronomers, however, do not agree with his conclusion. They attribute any supposed change in brightness and extension to the unusual illumination produced by the new star.

On December 13, 1885, Mr. Gore, of Beltra, Ireland, observed a star of about the sixth magnitude following X′ *Orionis* by $1^m\ 25^s$ and about 6′ north of it. X′ *Orionis* is a fifth magnitude star in the club of Orion. He could not find it in any of the catalogues and suspected it to be a temporary. It was soon acknowledged by all astronomers to be a new body. It lost light from the time of its discovery and early in July, 1886, it became invisible even in telescopes of moderate size. But in August it again became visible and increased slowly in brilliancy.

This speedy return to visibility and continuous variability indicate that the star is not a temporary, but a hitherto unknown variable. Its maximum magnitude

must be about the sixth, its minimum lower than the twelfth. Its minimum occurred in July and its maximum probably about the time of its discovery, December 13, 1885. The question naturally arises, "How has this star escaped detection, since good star catalogues contain stars as low as the tenth in magnitude?" The most probable explanation is, that at those periods when this portion of the heavens was examined by the various astronomers who have built up catalogues, the star was passing through its minimum. Duner assumed for the time being that the period is a little less than a year, and he placed the maximum at 1885, December 15, plus three hundred fifty-nine days. On this basis, he showed that at the time, when these Orion stars were being catalogued by Laland, Bessel and Argelander, the newly-discovered star was considerably removed from its maximum brightness. The last maximum occurred on or about December 12, 1886; therefore its period appears to be somewhat less than a year, as Duner supposed. The Potsdam observer thinks its color at the last maximum was less ruddy than at its discovery.

I have been observing the *Nova Orionis* at varying intervals since its last maximum. It is now about the 8.6 magnitude, a red star, clear and sharp in definition. Early in February I could not detect any change for several days. Müller, at Potsdam, noticed a similar break in the decrease of light at about the same time last year; also another during March.

Without doubt, therefore, the *Nova Orionis* is a variable and not a veritable new star. But what is the *Nova Andromedæ?* This question is not so easily answered. We must call it a temporary according to our definition, but is it of like nature with *T Coronæ* and *V Cygni?* Spectrum analysis must decide this question, if decided at all.

At the risk of going over familiar ground I will review briefly the spectra of stars as now classified by astronomers. Secchi who was among the first to apply spectroscopic investigation to the stars, divided the numerous spectra examined by him into four classes. The first includes white stars mainly, like Sirius, which give spectra with a marked predominance of hydrogen lines, all others being comparatively inconspicuous. The second includes mainly yellow stars and give spectra like that of our sun, such as Capella and Pollux. The third includes mainly red and orange-red stars, like Betelgeuse, which give spectra with several prominent absorption bands *i.e.* they are bands, not lines like the ordinary solar spectrum lines. These bands are sharply defined toward the violet end and shade off toward the red end. The fourth class comprises small red stars, none brighter than the fifth magnitude, which also give banded spectra. But the bands are in this case fewer in number, (three) broader, and sharply defined toward the red end, shading off toward the violet; in this last respect just the reverse of the Betelgeuse spectrum. A representative star of the fourth class is a 5.5 magnitude star in the Great Bear, called by Secchi *La Superba* from the brilliancy of its prismatic colors.

Prof. Vogel, of Potsdam, who has devoted careful study to this subject, has decided to put Secchi's classes III and IV into one, which he calls III_a and III_b. He believes they denote the same stage of development in order of time. He holds that the first type belongs to bodies at a maximum temperature, having few elements except hydrogen in their atmosphere at a sufficiently low temperature to produce absorption lines. The second type implies a lower temperature, which has produced an atmosphere in the condition of that surrounding our sun. The third comprises bodies whose atmospheres have become heavily absorptive; and III_b

differs from III_a, not in the order of stellar existence, Vogel thinks, but in the constituency of atmosphere. What substance or substances produce the absorption bands of III_a neither Vogel nor other investigators are ready to conjecture, though they incline to believe that they are attributable to one substance rather than to several. The three bands of the III_b type, suggest strongly carbon compounds. Similar bands have been seen in the spectra of comets, notibly the large one of 1882. Vogel has published (1883), a carefully prepared catalogue of star spectra. His catalogue includes four thousand fifty stars extending to the seventh magnitude. Duner, of Lund, Sweden, has published (1884) still another catalogue of spectra of the class III_a and III_b, containing about three hundred fifty stars, forty-five coming under III_b. It is a significant fact that no one of the III_b type yet discovered is larger than the fifth magnitude. It may be that they are in themselves neither smaller nor less brilliant than other stars, but that their heavy atmosphere cut off the larger part of the light which would otherwise radiate to us.

Nearly all the variable stars (excepting those of the Algol order), fall into the third class.

Now, what does the spectroscope tell us in regard to the *Nova Orionis?* It confirms our conclusion that it is an ordinary variable. Duner says, "Its spectrum is a fine III_a with broad and dark absorption bands"; Vogel, of Potsdam, "It is a beautiful, clearly-defined spectrum of class III_a with many dark bands"; and Wolf, of Heidelberg, "It is a pure III_a spectrum and the star is without question a variable of the *Mira Ceti* class."

We must now ask, what are the spectra of temporary stars, to which division the *Nova Andromedæ* must belong if we attempt to classify it at all? The new star of 1866 was the first one examined by the spectroscope. Huggins obtained its spectrum about four days after its

discovery. It gave what is called a double spectrum. Upon a background of a III_a, there appeared four bright lines, two of which coincided plainly with hydrogen lines. The spectrum of the *Nova Cygni* was examined shortly after its discovery both at Paris and Potsdam, and was found to be very like that of the Corona star, bright lines (indicating hydrogen) on a continuous background. This spectrum implies a sudden and enormous outburst of luminous hydrogen; corresponding, perhaps, to the red prominences of our sun, though necessarily exceeding them vastly in number and extent.

These bright lines were discernible after the continuous spectrum upon which they lay was no longer seen. They were followed until the star, in the case of *V Cygni*, had fallen to the tenth magnitude, when but one line in the green could be detected. This green line is the one prominent in the spectra of gaseous nebulæ. This would suggest that we may have here a case of reversion to the nebulous state. Prof. Proctor, who believes that new stars have some intimate connection with nebulæ, assumes that a small planetary nebula, hitherto unnoted, exists in this place, and that the green line indicates only the comparatively permanent conditions under which the temporary presented itself. But Prof. Pickering says, in his annual report of 1880, that the spectrum of the *Nova Cygni* has become that of an ordinary star, though exceedingly faint.

The two new stars, therefore, whose spectra have been examined are characterized by bright lines on a continuous background.

Of the nebulæ whose spectra have been examined (mainly by Huggins), about one-third give only bright lines and thus proclaim themselves to be of a gaseous nature. The Andromeda nebula does not fall into this third. It gives a continuous spectrum, without visible lines and with the red end wanting.

The spectrum of the new star was also continuous, without lines, dark or bright; and according to many authorities truncated at the red end. All observers do not agree in this statement, that the red end was wanting, at least, all do not mention it. Dr. Copeland, of Dun Echt observatory, speaks of it as "quite continuous" without qualification. All agree, however, that it much resembles that of the great nebula in which it lay. Dr. Copeland thought at times that he saw glimmerings of bright lines, but he used no stronger expression than "suspicion of bright lines."

The spectrum of the *Nova Andromedæ*, therefore, gives no evidence of a special illumination of hydrogen, such as characterized *T Coronæ* and *V Cygni*. Moreover, the similarity between the spectra of nebula and star suggests a real connection between the two. But many astronomers do not accept this correspondence as conclusive, and incline to the opinion that the Nova is a temporary lying in the direction of the nebula only, and having no connection with it. Within the area covered by the nebula lie more than a thousand small stars, and one of these, according to their view, has proved itself a temporary. Hasselberg, of Pulkowa, holds that the want of bright lines in the spectrum is not sufficient cause for throwing the star out of the category of the *Nova Coronæ* and *Nova Cygni*.

It indicates, he thinks, a difference of internal constitution rather than different causes inducing the outburst. He adds, that if it has a physical connection, it ought to be within the nucleus of the nebula. I do not see a sufficient reason for this statement. Further, there should be some visible change in the nebula itself, which change careful observation does not substantiate. This is an objection of some weight, though it should be remembered that there is extreme difficulty in determining the outlines of a nebula, good observers differing

among themselves in their drawings; furthermore, that the appearance of a bright body in its midst would of itself obscure minor changes.

Other astronomers hold that a physical connection between star and nebula is the more reasonable conclusion; and I incline toward this theory in spite of the fact that many of the best authorities are not disposed to favor it. The appearance of a new star in 1860 within the Scorpio nebula, to which I have already alluded, gives greater import to this supposition. Is it probable that so rare a phenomenon as a new star should present itself twice in twenty-five years within a nebula, and in a nebula known in one case to be resolvable, and in the other supposed to be so upon spectroscopic evidence,—is it probable that this should twice occur and yet that the connection should be accidental only? Herr Seeliger, of Munich, who believes the connection is a real one, advances the hypothesis that the phenomenon may be attributed to a collision between two of the supposed constituent members of the close group forming the nebula. He takes up the problem mathematically and constructs a formula which gives the rate at which a body brought suddenly to a great heat by collision would lose light; and he finds for himself a satisfactory agreement between the theoretical values obtained by this process and the values obtained by observation. But the broad assumption of data necessary to the deduction of these theoretical values detracts from the importance of such evidence. Still, in a close group collision is not improbable, and perhaps quite as probable as that there should occur in this long observed nebula, unchanged within recording epochs, so sudden a transformation from causes lying within the bodies themselves.

It is, of course, possible that other temporary stars may have been caused by collision. Sir John Herschel

has noted that all new stars, whose positions have been accurately recorded, have appeared within or near the milky way.

Another theory, kindred to Seeliger's, has been promulgated by Mr. Monck, of Ireland. He suggests that the star may be one of those swiftly moving bodies, like "the runaway star" of *Newcomb's Popular Astronomy* (1830 Groombridge), which has come in contact with the nebula, and has been set on fire as a meteor is ignited in traversing our atmosphere. Prof. Hall calls this theory "ingenious." It appears to regard the nebula as gaseous, and that it may be partly gaseous is quite generally admitted. If it was a runaway star suddenly brought to an enormous illumination by contact with the nebula, it would probably have shown some parallax. Prof. Hall could find no evidence of change of position, at least, none which he regarded as trustworthy.

Therefore, the classification of the *Nova Andromedæ* must remain for the present unsettled. If the new stars of the future present spectra rarely failing in bright lines, and if the exceptions occur when brilliant bodies suddenly blaze out in the midst of resolvable nebulæ, astronomers will have firmer ground for the building up of theories. Certainly the data are now insufficient, and we can only surmise in regard to this interesting development within the great nebula of Andromeda.

The chairman, Dr. Stevenson, extended to Miss Whitney the cordial thanks of the Section for her interesting and instructive paper, the subject of which was still further discussed by Messrs. Warring, Cooley, Bristol, Loomis, Dwight and Stevenson.

MARCH 23, 1887—FIFTY-FIFTH REGULAR MEETING.

William G. Stevenson, M. D., chairman, presiding; thirty members and guests present.

The following paper was read:

THE EVOLUTION OF CONTINENTS.

BY CHARLES B. WARRING, Ph.D.

The evolution of which I shall speak to-night, has nothing to do with the formation of mountains, valleys and plains, the surface features of the continents. It preceded them, and bears to them the relation that the foundation does to a building. It presented the same problems ages before the first mountain was lifted, or the first valley eroded, which it presents to-day.

If the water could be removed from the oceans, the land would appear as two immense plateaux, with numerous smaller ones, occupying all together about sixty million square miles. These plateaux would be seen to rise quite abruptly ten thousand feet and more above the great valleys which separate them, and which are now the receptacles of nearly all the water on the globe. The existence of such enormous masses rising to such a great height above the general level, in apparent violation of that law of hydrostatics—the surface of fluids must be level—presents the first of our problems, for although the continents and ocean bottoms are solid enough now, there was a time when they were fluid. Then, of course, the surface was level; the problem is how to get from the level surface to one presenting such immense elevations.

Another problem confronts us when we consider the contour of the land. There is a remarkable parallelism between the shores of the Atlantic, while the opposite is true of the shores of the Pacific. If we could move South America over to Africa, its eastern angle would fit surprisingly into the gulf of Guinea, and the two coasts thence southward would almost coincide. The great western projection of Africa fits into the recess between North and South America. Thence northward

the parallelism is almost perfect, Labrador and New Foundland fitting into the bay of Biscay, while the plateau on which are the British islands, and whose boundary is the one hundred fathom line, fits with some approach to accuracy into the opening of Baffin's bay. These peculiarities are well shown on the map of the world on Mercator's projection, which is found in all modern school atlases.

FIG. 1.

Looking at other parts of the world, we notice several other parallelisms. New Zealand on its western shore is almost the exact reverse of Australia, although one thousand miles away. Tasmania fits pretty well on the southern corner of Australia, and Papua, or New Guinea, on the north side. Madagascar agrees better with the north side of Australia than with the coast of Africa. The east and west shores of Baffin's bay are singularly parallel.

The trend of the island system of the southwestern Pacific is very suggestive. The diagram copied from *Dana's Manual of Geology*, with the addition of Australia and a part of Asia in place, shows this very clearly.

To these problems a third must be added :

The rectangularity of the coast of the Pacific, the opposite of the parallelism of the Atlantic. The trend of the eastern side of Asia instead of being parallel to the western side of America, is almost perpendicular to it.

In regard to the literature of the subject, I have been able to find very little. The belief of scientists in a general way, appears to be, that as our earth cooled, a crust was formed all over it, with tolerable uniformity ; that as the interior cooled and shrank, the immense weight of the crust produced corrugations on its surface, and that these were, from some cause, not generally distributed, but were in huge patches, the largest of these being what we now call the eastern and western continents. This would account for a portion of the earth's crust being higher than the rest, but not for the direction of the trends.

Prof. Dana, in his *Manual of Geology*, (third edition, page 816), offers the following :

The continents were the parts of the earth's crust that first stiffened in the gradual cooling of the exterior. These parts sank as they solidified, and of course there was an overflow from either side, which also cooled. And this process went on until the mass became too viscid for it to continue. Finally a layer of crust-rock, miles in thickness, was made over the great continental areas. Throughout the other portions of the sphere, the surface, whether liquid or beginning to solidify, had one level. The earth then was one vast plain, the present continental portions solid, the oceans liquid. These liquid portions, as they cooled, would shrink more than the solid parts ; hence, as they hardened into rock

they would form vast depressions, and these now filled with water are the present oceans. He adds; The cooling of one part of the crust before the rest must have been a consequence of less vivid heat and violent action in the liquid rock of that part, and this, he says, may have been connected with the exterior of the solid nucleus, being nearer in that part than elsewhere to the outer surface of the sphere, that is to there being less depth of liquid rock over it to cool.

To this explanation two objections present themselves. It is, I think, extremely doubtful whether solid granite would sink in the molten mass, a doubt that becomes almost certainty, when we reflect that astronomy has proved that the interior of the earth is very much heavier than its crust. But admitting that, what Prof. Dana says, explains the existence of the vast ocean basin which now separates the continents, it does not even attempt to explain the parallelisms which are found, except to refer them to lines of cleavage existing in the earth's crust. It is difficult to see how lines of cleavage could affect coasts which had always been thousands of miles out of reach of each other. If cleavage planes exist, they could easily produce their full effect without the parallelism which we now see. South America, for example, might have had that projection which now terminates at cape St. Roque, turned equally far to the west of the meridian which runs from cape Horn to the gulf of Maracaibo. The coast lines would remain parallel to the same great circles as now, only in a quite different order, and the parallelism between South America and Africa—the thing for which we are seeking a cause—would disappear.

Humboldt offers a very different explanation. It is that the Atlantic basin was eroded by an ocean current. The present Atlantic currents are too feeble and too superficial to produce any erosive effect on the bottom

of the basin. But, if in some way the power of the current was then enough greater to do the work, we are met with the question: How was the débris disposed of? There would need to be removed a mass exceeding in bulk North and South America and Europe combined. What became of it? No satisfactory answer has been given. For this, among other reasons, this theory has not been adopted by scientists.

The fact was long ago pointed out that the great trend lines of the land are approximately tangent to the arctic circle, and hence it has been thought that in some occult way the direction of the coast lines was connected with the obliquity of the earth's axis. It is very difficult to see what the connection can be, and until some one shows what it is, I think we must be content to regard the tangency as an accidental coincidence.

In the *Popular Science Monthly*, of a few months ago, are two articles by Prof. Dawson on the geology of the ocean. His paper however, begins at a time long subsequent to the development of the continental forms, and whatever light it may cast upon changes that have occurred since the waters descended and filled the ocean basins, it casts none upon all that went before.

So far as I can see, we are left to our own unaided efforts to solve these problems in world-building.

The unknown always excites our interest and piques our curiosity. Nor is it presumptious to try, where others have failed. Success will come only after many abortive efforts. In no other way can the secrets of nature be wrested from her. And so, without further apology, I will lay before you, what seems to me a partial solution of the mystery of continental elevations and of the trend of the coast lines.

In the study of these questions, I started with the assumption that the laws which governed the formation of the continent are laws which are in force to-day. I

found nothing to oppose, but everything to sustain the consensus of present geologists that the elevation of the land occurred at an exceedingly remote period. It came in the wide interval which separated the period at which the nebular hypothesis ends, from that at which geology begins. The former brings us to a globe of molten lava; the latter starts out with a world possessing a solid crust, covered with an ocean beneath whose surface lay the land hidden by a few hundred feet of water.

In the first part of this interval the continents were formed. The earth from pole to pole was an unbroken sea of lava exceeding in temperature and consequent fluidity, the lavas now glowing in the most intense action of present volcanoes. It was in a state of violent agitation; there were not only convective currents in its own mass, but it was acted upon by an atmosphere probably thousands of times heavier than that which we breathe, since it contained all the waters now on or in the globe, all the carbonic acid now in the rocks as carbonates, or whose carbon is now found in the various forms of coal, lignite, or hydro-carbons, as well as all else that was vaporizable at such high temperature.

This ocean of fire was a mixture of all the elements, substances differing enormously in fusibility, and in specific gravity. It is highly probable that a very large proportion was iron—for our earth possesses magnetic properties, and in that resembles iron. As a whole it is very much heavier than the continental masses. If two-thirds of the globe were iron, it would have about its present specific gravity. In the igneous rocks, Prof. Dana says, there is a high per cent. of iron oxyde, and he says, too, that the greatest iron ore beds of the world are found in the oldest rocks, the Archæan.

As gases are vacua to each other, so in a less degree are liquids, and it must be admitted that there was a more or less general permeation of substance through

substance, yet in a general way, it is true that the heavier substances were found below, and the lighter near the surface. As the temperature fell, the more infusible substances solidified first, and, as these, as a class, are lighter than, for example, the iron which with other materials made up the magma, they rose as a scum, or crust, to the top, a process which we see repeated on a minute scale, at every smelting furnace, in the slag which floats on the molten metal.

The slag or crust, formed on the lava would gradually increase in thickness very much as the ice increases on the surface of a pond. Twenty, thirty, or a hundred miles would not be very thick for such a formation. Such a process, if undisturbed, would have covered the earth uniformly — substantially so — with a crust of present rock-materials,— very much as ice covers, and floats on, the surface of a pond.

As the interior of the globe continued to lose heat, consequent contraction went on, which tended to produce elevations and depressions in the floating crust. wrinkles on its surface with as little system as those on a withered apple. If undisturbed, such might now be its condition ; but other forces were at work tending to break up the crust, and thus to prepare it for rearrangement in one or more masses. There were the lunar tides, and in a less degree the solar tides. These in seas thousands of miles in depth, must have been much greater than ocean tides now. Their effect was to weaken, by frequent flexure, the crust, and, to a certain degree, to cause a relative movement westward. There were also enormous eruptions of vapors and gases such as those now occurring in the sun. And there were seismic forces, probably incomparably greater than those now in operation. To these we may with probability add the influence of cyclones which in such an atmos-

phere as then existed, and with such enormous temperatures, must have been exceedingly destructive.

Another important force came from the rotation of the earth. Every one that has seen water fly from the face of a grindstone, has noticed, when the centre of the face was the highest part, the water heaped up there before it began to fly off. For similar reasons the floating ing débris moved toward the equator. As in every cauldron of boiling matter the scum takes a form peculiar to itself; so here, the scum, or slag, took some form, it may have been perhaps a girdle, or equatorial ring, and in some cases it might remain a ring, and so perhaps it has, in other worlds, but in ours, it did not; it was broken in some way, and probably by the disruptive forces of which I have spoken. Then began a new process; there was a drawing together of the ring by its own attraction. This probably occurred when the ring was yet narrow, and while yet a great part of the floating scoriæ had not been gathered in. Around the poles, or rather at the poles, the centrifugal force was zero. Going from them, it increased slowly, reaching its maximum effect in latitude 45°. Hence there were probably large masses around the poles even after the ring had been formed. If fragments disrupted by seismic forces, floated to the central mass, and chanced to strike it near its southwestern and northwestern shoulders, they would give it a rude, triangular form which subsequent additions might readily develop into the great triangular mass which seems to have been the antecedent of the present continental masses.

This immense floating island had a slow westward motion caused by the convective currents. The upper or surface material descending, lost a part of its eastward motion, and when it returned to the surface, it came with a diminished eastward motion, and hence, in reference to the earth as a whole, it had a slow west-

ward movement, and carried with it whatever floated on its bosom.

Another fact, important in this connection, remains to be spoken of. For many reasons it it probable that, owing to the great pressure, a large part of the earth's interior solidified, or at least became pasty and viscid before there was any crust on its surface. This solid interior was not wholly at rest, there were elevations and depressions on its surface, for it seems to be a law of nature that upward movements occur in all bodies passing through the stages of cosmic development. The study of the nebulous mass from which sprang the solar system leads to the same conclusion. Then there are the irregularities in the forms of present nebulæ, the frequent upheavals in the sun; the relatively greater movements which in recent times have caused the "shoulders" of Saturn to rise more than one thousand miles; the great uprisings which have occurred in our earth since geology began, all combine to establish the fact that vast irregular upward movements are a part of the plan of world-building. There is, then, so far as I can see, nothing to forbid the belief—but much to establish it—that portions of the solid nucleus rose above the rest.

If, then, there existed a triangular mass such as I have described, which moved slowly westward, we may think of it as a great cake of ice floating on a river of very moderate velocity, and obedient to the same laws, and subject to the same accidents as other cakes of the same material. If, when passing over some shallow spot, it lodged upon a shoal or elevation of small area, but too large and firm to be pushed aside, and situated on one side of the axis of the mass, the impetus previously gained, aided by the current, would cause a partial, or complete,—according to circumstances—rotation around the fixed point. We may then believe that if

while the mass rudely represented in the condensed chart (fig. 2), was moving westward, it chanced to strike its

Fig. 2 represents the great triangular mass soon after seismic forces began to rend it in pieces and before the eastern portion began to veer around northward. Of course at this time there were neither mountains nor rivers.

FIG. 2.
A condensed Mercator's Chart.

lower surface—to run aground—on such a shoal situated, for example, under what we now call the Soudan, the eastern portion would veer around northward, and would continue to move in this direction until it either ran aground itself, or met some obstacle.

Inconceivably great seismic forces were at work, and from what we know of their action in historic times, we may easily believe that a crack or rift might go in a sort of zig-zag across the mass. We have seen cakes of ice break when floating along, and we all have noticed that however crooked the line of fracture, its sides at first are parallel, and remain so until the fragments are by some means turned more or less around.

In this immense telluric cake of such ice, the pieces beyond the line of separation would be floated westward. And when the northwestern corner of the upper fragment came close to the end of the eastern part, which it will be remembered we left moving northward, both would be brought to rest, and thus would be produced the perpendicularity of the North Pacific coasts. The lower end would also be brought to rest should a similar shoal be formed under what is now Mexico. Like causes would result in South America's floating to its present position, and thus would be produced the parallelism of the Atlantic.

Given then a solidified mass of scoriæ—in form rudely triangular—floating westward in the fluid matter, there would be needed only two or three elevated spots on the solid, or semi-solid nucleus, to give, with the aid of disruptive, seismic forces, the present arrangement of the four grand continental divisions.[1]

I have said that, owing to the absence of centrifugal force at the poles, it was quite possible that a mass of crust remained a longer or shorter time about each. As to the northern region, it seems all to have been broken

1 Europe and Asia being really only one mass.

up and drifted away, and if this occurred after North America and Asia got into position, it is easy to see that the fragments would be more or less perfectly welded into them. The remarkable archipelago north of North America is very suggestive of such a result.

At the southern pole, the case appears to have been different. The disruptive, seismic force does not seem to have split the antarctic continent near its centre, but rather to have chipped off fragments of various sizes. These would naturally float off with the northwesterly currents till they, too, grounded at any place where they reached an elevated spot on the solid nucleus. If we turn to a polar projection of the southern hemisphere, and compare Australia with the antarctic continent, the parallelism of form readily suggests the belief that they were once united, and that Australia being broken off, was carried like a vast iceberg northwestwardly till, having left New Zealand fast on a shoal, it came itself to rest, a thousand miles further on, from a similar cause.

Such a mass, grounding directly in the stream, would cause the current to swerve, the upper part passing between it and Asia. Now, with this in view, turn to the chart (fig. 1), the resemblance to floating cakes of ice grounded where the stream is shallow, is almost irresistably suggested, or perhaps it would be better to say that the stream by loss of heat had grown too viscid to permit the fragments to go further, and that they had chilled fast. Madagascar may have been another fragment which left Australia, and floated westward till it floated into the shallows near the coast of Africa, and thus we find a reason for the curious fact that the eastern shore of that island fits much better against the western shore of Australia than does the other shore against Africa. Papua, too, and Tasmania appear to be fragments broken off from the adjacent shores of Australia.

Further study of the chart suggests as at least possible, that in the sweeping around of Asia towards what is now Behring's straits, the great strain caused the crust to crack apart from Suez to the straits of Babel-Mandeb, thus preparing a place for the Red sea. Under the great vertical pressure, the crust at the depth of a few hundred, or perhaps thousand feet, in its then intensely hot and semi-plastic condition, would naturally have bulged out and so filled up the trough from that point to the liquid below. The basins of other seas may have been formed in similar manner. It is not difficult to imagine some hitch in the process by which Europe and Africa were separated and then, the basin partly filled by the in-pressing of its sides, as clay is now sometimes squeezed out of its bed by the superincumbent masses. If so, then the existence of the Mediterranean would be accounted for. It is easy to apply this reasoning to other seas. But what has been said will suffice for the present.

Suppose, then, that the breaking up of the great mass of crust has ended and that all the fragments are securely anchored. The four largest are the substructure of the present continents. The others are the antecedents of a part of the present islands, either separate, or in groups. Around them at that time instead of water as now, was an ocean of liquid rock. I cannot think with Prof. Dana that the surfaces of these plateaux and of the intervening seas of lava were on a level. The difference between the specific gravity of the slag and of the magma from which it was segregated, must have caused a corresponding difference of level. But it is not probable that the plateaux rose as high above the lava, as the present land rises above the ocean bottom.

As cooling went on, the fluid matter contracted more than the solid, and consequently the difference between

them would increase, and, in fact, it has increased to its present limit.

It is absolutely essential to this theory, that land and sea should never have changed places, a requirement which not very long ago would have been thought fatal to any theory, but which is now in accord with the belief of nearly all scientists.

While I have been reading this paper, perhaps your thoughts have passed to other parts of the universe to inquire how facts elsewhere corroborate the theory which I have presented.

There is little that we know of processes outside of our planet. It is, however, at least a suggestive fact that our earth, which was a miniature sun when the scoria was gathering into a mass, was also a variable star, a subject on which we had so interesting a paper at our last meeting. At that time all the present ocean bottom was glowing with heat and light. To an observer at Mars, the earth would present a variation of intensity in its light, going through its period in about twenty-four hours. As the scoriaceous mass was, roughly speaking, an acute angled triangle with its vertex to the east, and its base to the west, the light diminished gradually, but increased with much greater rapidity. The mode and rate of variation would depend upon the form of the floating mass, —the spot as it would appear to the distant observer. The variation would be very great since the spot, at first there would be but one spot, in its most favorable position, would intercept rather more than one half of the light, and, in twelve hours more, intercept none at all. When afterwards, the great spot broke in two, and a part floated three thousand miles away, there would be two maxima of unequal intensity and two minima, also of unequal intensity. The greatest maximum would be when what is now the Pacific basin was turned to the observer; this would be followed by the

greatest minimum when what is now the eastern continent came under his eye; then, as the Atlantic basin swept along, there would be a second but smaller and briefer maximum, itself followed by a second minimum as the non-luminous spot which we now call the western continent swept into view. This seems almost like the description of β Lyrae (I quote from *Newcomb's Astronomy*, page 429). "That star has two maxima and minima of unequal brilliancy. Should we observe it to-night, we might find it of the four and one-half magnitude; then in the course of three days it rises to the three and one-half magnitude. It then begins to get smaller, and comes down to the fourth; once more it begins to grow brighter, and rises to three and one-half, and again it diminishes to its first size. The whole variation requires thirteen days, and is divided into two unequal portions of six and seven days each." In reference to the variations being caused by dark stars, he said: "In Algol there seems to be only one spot. About once in three days it fades from the two and one-half magnitude to the fourth, and remains there a few hours. This change cannot be due to a dark star passing between us and Algol, for the latter does not lose its light and regain it in the same time, which it would do if the change was caused in that manner."

What may be the real cause of the changes in the variable stars I am unable to say. Probably in general, they are due to spots such as those seen on our sun, yet the great persistence of some of them suggests something different, as the greatest duration of solar spots is only a matter of a few weeks—possibly months.

The moon and Mars are relatively near us, and it is possible to study their features. It may be said that as neither presents features such as the earth, although they, too, have passed from the molten to the solid

state, therefore, such an arrangement as we see in our world was not due to the causes which I have assigned. But this proves too much. It assumes that these causes should always result in the same, or similar forms. In the breaking up of an ice field, we say in general terms that the same forces are working everywhere, there is the current, and there are the winds, and there is the heat of the sun, yet in what an infinite variety of shapes, do these result. Scarce two pieces will be alike. So I should expect each globe would, in its cooling and solidifying, work out results peculiar to itself.

I see nothing therefore in the condition of the moon, or of Mars, that opposes the explanation which I have offered. Through some such process, I have little doubt, the grand features of our planet were brought into their present form. After that the cooling still went on. The bottom of the ocean basin after a sufficient time ceased to glow, the falling temperature caused the hitherto invisible, super-heated vapor to take visible form as clouds, or to descend in torrents, to be again and again flung back. The conflict went on, the oceans filling up, until at last, the waters spread over the vast plateaux, covering them all to the depth of a few hundred feet. The hot seas grew cooler; life in its humblest forms was introduced, and the dry land only waited for the fiat that was to bid it rise above the waters.

The rest of the story of the continents is told in the record left in the rocks, and geologists have deciphered it for us.

In the discussion which followed this paper, participated in by Messrs. Dwight, Cooley and Stevenson, Prof. Dwight said that the theory of continent formation just advanced, avowedly left out of consideration

the facts of the extensive foldings of the strata by lateral thrust into hills and mountains, as being irrelevant to the question. So far as the more recent uplifts are concerned, such as those of the Rocky mountains, this would be doubtless true. But the consideration of the extensive folding of the very lowest foundation-strata of the continent, in the very early ages of the growth of continents, cannot properly be left out; that is, the folding of the Archæan rocks. There is not a spot on our continent, where this universally underlying stratum is not folded up. In mountainous regions, the lateral compression has been estimated by Prof. LeConte to be in the proportion of five to two; that is, what was originally five miles in breadth is now reduced to two miles in breadth. Supposing the average contraction in breadth by the universal folding of the Archæan to be much less, say one half of the above ratio, it would indicate an enormous action of forces in modifying the shape of the entire continent.

Now the theory advanced supposes, for instance, that the Atlantic coast lines of North and South America retain corresponding shapes to those of the Atlantic coast lines of Europe and Africa from which they broke away when the entire American continent floated to its present position. If we may suppose this great raft of floating rock to have navigated unbroken for three thousand miles and more, and then to have become permanently anchored, then it is absolutely necessary for us to assume that the folding of these foundation strata must have been done by two vast forces acting exactly in parallelism to the two great trends of the coast lines. Otherwise the original form of the continent could not have been preserved, as the present theory supposes. But if we find it necessary to suppose these two great forces acting thus at right angles to each other and parallel each to each to the Atlantic and Pacific coasts,

after the continents have been shaped and finally located by this flotation theory, then there is no need of this latter theory at all. For such forces acting as described are quite sufficient to elevate and shape the continents without the necessity of our supposing any such daring scheme of a continent-raft.

In closing the discussion Dr. Warring remarked that the folding of the rocks was an effect not yet sufficiently studied to enable us to say how great it was, but that he doubted very much its being as great as had been said, viz: that every five miles in breadth had been reduced to four, or in other words the continents which now measured, roughly speaking, twelve thousand miles from east to west, once measured fifteen thousand. This, however, was to be settled, not by *a priori* reasoning, but by measurements. But whatever the size of the folds, the question of importance in the present connection was the direction of the force producing them. Forces parallel to the coasts would not effect their trend, neither could they produce folds; such forces therefore may be dismissed from consideration. Two facts stand out prominently; one is that the strikes of, say, the Archæan folds are parallel to each other, and to the adjoining mountain system, and the latter parallel to the trend of the coast; and the other fact is, that the direction of the fold-producing force must have been perpendicular to the same lines, *i. e.*, to the strikes, and to the coasts. What, then, would be the effect of such action? It would result in a movement of the coast line, pressing it inward, *i. e.*, toward the axis of continent, but with little disturbance of its trend. If every point in, e. g., a north and south line, be pushed westward by a force applied perpendicularly, the line will move westward but will remain parallel to its first position. This is equally true whatever the direction of the line. Hence if two lines so acted upon were parallel

before the application of these forces, they will remain parallel after it. The same is true of a broken line or of a curve.

Reasoning backward, we may say that the parallelism now existing, proves parallelism before the play of the forces which caused the folds. But if it be said, that this would require those pushing forces—so called for lack of a better name—to be approximately equal, or at least that they did not differ greatly among themselves, it may be granted, and indeed is what might be expected if the thrust came from the diminishing temperature of the ocean bottom, since that is approximately uniform in width.

Taking into consideration that according to geologists (See *Dana Man. Geol.*) the trend of the coasts have undergone no essential change since the earliest date of which they have any knowledge, and the effect of forces perpendicular to the coast trend, it is difficult to see how we escape the "need of this flotation theory," at least till something better is devised than has yet been offered.

APRIL 20. 1887—FIFTY-SIXTH REGULAR MEETING.

William G. Stevenson, M.D., chairman, presiding.

Messrs. H. Zingg, W. J. Bolton and Charles C. Mills were elected members.

The proposed amendments to the by-laws were adopted.

The following paper was read:

THE CUTTING AT CROTON POINT, N. Y.

BY CHARLES B. WARRING, Ph.D.

Just below Croton Landing, thirty-three miles from New York, is a remarkable point or spit of land nearly two miles in length, and separating Haverstraw bay

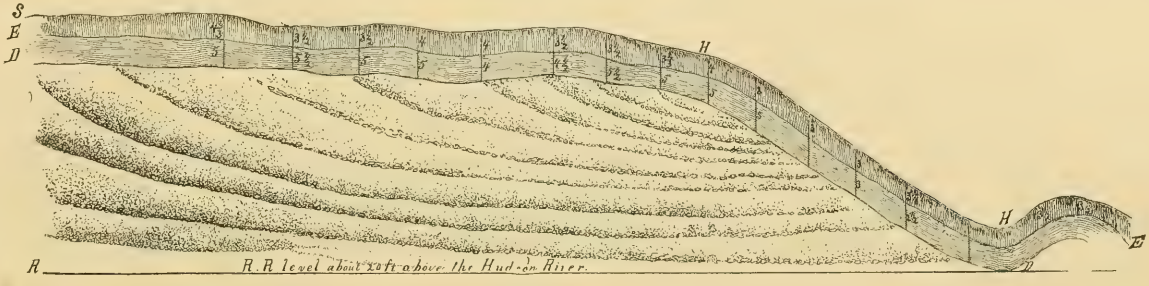
from Tappan Zee. It is noted for the excellence of its fruit and other productions, and especially for its superior grapes. The surface is almost level, bounded by wooded slopes, which are everywhere steep and in some parts almost perpendicular, as perpendicular as is possible for sand and clay to lie. I do not think there is any bed rock visible, at least I have seen none. The point is divided into two parts, separated by a morass now covered with salt meadow. From seventy feet above the water the surface descends very abruptly to its level. Crossing the morass, the land rises in the same manner nearly or quite to the same height. There is an island in the morass at the mouth of the passage through the point of about the same height as the rest, and its sides are equally steep.

Through the neck of this spit of land the Hudson river railroad company has made a deep cutting. About two years ago they drew away many thousand yards of material for filling, and so removed the débris which had largely accumulated, and in fact they stripped the sides of the cutting bare from the bottom to top, thus revealing very beautifully its structure. The accompanying diagram was made within a few weeks of that time. The measurements were made with a twelve-foot pole let down from the surface by an assistant while the writer, standing in the cut below, and at a sufficient distance back, directed how far to put it down. The measurements were thirty feet apart. The Croton river comes into the Hudson a little to the left and south of the cutting.

It will be seen that there are three divisions in the construction of the bank. The first and largest consists of layers of very fine sand, having in the bottom part of each (and nowhere else), considerable coarse gravel at the south end, and more or less all along, but finer as the distance increased. These layers are sharply de-

WEST FACE OF CUTTING THROUGH CROTON POINT, THIRTY-THREE MILES NORTH OF NEW YORK.

The figures denote feet. The perpendiculars are thirty feet apart. Length of cutting is about eight hundred feet.



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The largest portion, R to D, is made up of layers well defined, very much wider at the left (or south end) and diminishing towards the north. Each layer is composed at bottom of cobble stones (2 to 6 inches in diameter) and gradually growing less till for the last quarter of the distance there are few stones as large as an inch in diameter. Immediately on this coarse material, and occupying four-fifths of the layer, is very fine sand.

From D to E are many well-defined layers running over the width of the cutting from E to E. In these is neither sand nor gravel. Apparently there was a succession of deposits in quiet waters.

From E to the surface, there seems to be only one deposit; at least there are no lines of stratification.

fined, ending abruptly as in the diagram. They do not die out at either end but are fairly represented. Lying next above these is the second division, averaging four feet thick, and consisting of a dozen or so layers of uniform thickness, very regular and everywhere parallel to each other. Above this is another division of about the same thickness and parallel to it. It shows no trace of any layers but is homogeneous throughout. The material of both the last divisions is very fine and was deposited in very still water.

At the right of the larger elevation will be noticed a smaller one. The two upper divisions follow all the inequalities of the ground.

How the lower division was dressed off so sharply on the top and side it is hard to say. It could not have been so shaped, at least not on the sides, by floating ice; besides, ice would have been quite apt to float there larger or smaller stones and drop them. After this part was formed, if then the waters had become comparatively quiet, the fine material of the group of layers which make the second division, might have been deposited, times of greater activity alternating with those of rest, and, last of all, a very long continued condition such as now exists in the present bay, would suffice to deposit the broad top layer.

After that was completed, the whole region must have been elevated to its present position very rapidly, or else the fine material of the top, sides and the lower hillock, would not be so nearly of a thickness.

The point must have been several hundred feet under water, for these same layers can be traced upon the neighboring hills to that height. Everywhere they follow the inequalities of the surface, and so far as I have been able to observe, maintain nearly or quite the same thickness.

Great quantities of the shells of the common oyster are scattered over the point, but never, so far as I could find, below the upper part of the topmost layer. I have been able to find no other organic remains.

When did this occur?

That the last emergence was very recent is evident because the slopes are so little worn.

The point itself must have been the northern part of the delta of the Croton river.

By one of those unaccountable freaks, so often manifested by rivers flowing through alluvial soil, the lower half of the delta has been swept away.

The curator of the museum, Prof. Dwight, read the following report:

To the Scientific Section of Vassar Brothers Institute: Since the last report the following specimens have been added to the museum by purchase:

Two specimens of Malachite and Azurite.
One specimen of Limonite.
One specimen of Galenite and Cerussite.
One specimen of Willemite and Franklinite.
One specimen of Sulphate of alumina.
One specimen of Fluorite coated with Siverite.
One specimen of Amazon stone.
One specimen of Cuprite with Limonite.
One specimen of Bornite with white quartz.
One specimen of Bronzite.
One specimen of Pyrolusite.
One specimen of Apatite.
One specimen of Fibrous Malachite.
One specimen of Rhodochrosite.
One specimen of Arizonite.
One specimen of Smoky quartz.
One specimen of Cerussite.
One specimen of Wavellite.
One specimen of Native Antimony.
One specimen of Chalcopyrite.
One specimen of Cuprite.
One specimen of Pyrargyrite.

Twenty-three specimens of polished Agates.
One specimen of Jasperized wood.
One specimen of polished Crocidolite.
One specimen of *Madrepora convexa.*
One specimen of *Madrepora spicifera.*
One specimen of *Madrepora* (species uncertain).
One specimen of *Mussa* (species uncertain).
One specimen of *Podabacia crustacea.*
One specimen of *Orbicella annularis.*
Three specimens of *Dichocœnia porcata.*
One specimen of *Herpetolitha limax.*
One specimen of *Ctenactis echinata.*
One specimen of *Fungia repanda.*
One specimen of *Meandrina clivosa.*
One specimen of *Pocillipora capitata.*
One specimen of *Tubipora syrniga.*
Three specimens of *Tubipora flabellum.*
Four specimens of *Turbo marmoratus.*
One specimen of *Haliotis rufescens.*
One specimen of *Haliotis gigantea.*
One specimen of *Voluta melo indicus.*
One specimen of *Voluta Junonia.*
One specimen of *Voluta* (species uncertain).
One specimen of *Voluta* (species uncertain).
One specimen of *Turbinella scolymus.*
Two specimens of *Fasciolaria gigantea.*
One specimen of *Fasciolaria distans.*
Two specimens of *Strombus gigas.*
One specimen of *Hippopus maculatus.*
Two specimens of *Tridacna squamosa.*
One specimen of *Triton variegatum.*
One specimen of *Cassis madagascariensis.*
One specimen of *Murex pomum.*
One specimen of *Onustus indicus.*
Two specimens of *Harpa* (species uncertain).
One specimen of *Spondylus imperialis.*
One specimen of *Oliva porphyria.*
One specimen of *Eburna areolata.*
One specimen of *Ranella crumena.*
One specimen of *Conus virgo.*
One specimen of *Conus striatus.*
One specimen of *Conus textile.*
Three specimens of *Conus eburneus.*
Three specimens of *Conus pulicarius.*
Three specimens of *Conus literatus.*

One specimen of *Conus* (species uncertain).
Three specimens of *Conus* (species uncertain).
One specimen of *Cypræa isabella.*
One specimen of *Cypræa africa.*
Two specimens of *Mitra episcopalis*
One specimen of *Nautilus pompilius*
One specimen of *Nautilus pompilius,* with section through the chambers.
One specimen of *Argonauta tuberculata.*
One specimen of *Limulus polyphemus,* female.
One specimen of *Libinia canaliculate,* spider crab.
One specimen of *Libinia* (species uncertain).
One specimen of *Asterias arenicola,* star fish.
One specimen of *Fulgur perversus,* egg-case.
One specimen of *Fulgur carica,* egg-case.
One specimen of *Fasciolaria gigantea,* egg-case.
One specimen of *Sciuropterus volucella,* flying-squirrel.
One specimen of *Tylosurus longirostris* (*marinus*), bill-fish or gar-fish.
One specimen of *Larus argentatus.*

The following donations have been received:

One specimen, native sheet copper, from lake Superior; donated by Dr. W. G Stevenson.

One specimen of limestone of the Potsdam group, containing the trilobites *Ptychoparia calcifera* and *P. Saratogensis* from Spackenkill road, Poughkeepsie; donated by Prof. W. B. Dwight.

One specimen of Quartzite, of the Olenellus group, or middle Cambrian, containing *Olenellus asaphoides* and other fossils, from Stissing mountain; donated by Prof. W. B. Dwight.

We are indebted to Dr. W. G. Stevenson for classifying and re-labeling the corals, shells, marine invertebrata, crustaceans, and vertebrate animals, and for many other kind services in the care of the museum.

Respectfully submitted,

WILLIAM B. DWIGHT,
Curator.

The chairman, Dr. Stevenson, read the following report relative to the work of the section during the year:

Members of the Scientific Section: It is gratifying to be able to report an undiminished interest in the work of the section. Its meetings have been well attended, and four additional members have been elected.

The earthquake which caused so great ruin at Charleston, and for a time occupied such general attention, very naturally suggested the subject of "Earthquakes" for your chairman's opening address on December 1, 1886.

On December 15, Prof. W. B. Dwight gave a detailed account—with illustrative specimens—of his "Recent Discoveries of Fossiliferous Cambrian strata in Dutchess County."

"From Coal Tar to the Alizarine Dyes" was the subject of an interesting address, January 12, 1887, by L. C. Cooley, Ph.D., and on February 9, Mr. C. L. Bristol reported his personal experiments in "Iodine Blowpiping." Miss Isabel Mulford, of Vassar college, read a paper February 23, on "Bacteria," wherein she gave the methods employed in their "culture," and described several species—believed to be new—which she had recently discovered ; and on March 9, Miss Mary W. Whitney, of Vassar college, read a paper entitled "New Stars in Andromeda and Orion," which have appeared within the last two years. On March 23, C. B. Warring, Ph.D., read a paper on "Continental Evolution," wherein he sought to explain, (1)—why the land rises above the ocean's bottom ; (2)—why the coasts of the Atlantic are parallel, and (3)—why those of the Pacific cut each other at right angles ; and on April 20 he described an interesting geological formation at Croton landing-on the Hudson.

By the amended by-laws of the Institute the museum and the library are placed under the immediate care of this section, and the curator and librarian are made officers herein. This change, it is believed, will prove beneficial, and the hope is expressed that the donations hereafter received for the museum and library will be more systematically registered, and be more freely used

to enhance the value of the section's work. However attractive specimens may be when classified and placed upon shelves in the museum, their real value depends upon their utilization for purposes of instruction; and to this end it is hoped that they may more frequently be made the subject of private or public study; by this means we may, at least, "promote useful knowledge" and thereby secure one important object for which this section was formed.

By an amended rule of the Institute the discussion of philosophical subjects is admissible in this section. In order, however, that such themes may not be permitted to unduly encroach upon our more legitimate scientific work, the suggestion is hereby made that a sub-section be formed wherein the study of philosophy alone may be pursued.

In asking to be relieved from further official service, your chairman desires to express his thanks for the uniform courtesy extended to him at all times, and to assure the members that, as a worker in the ranks, his efforts will not be lessened to promote the interest of the section.

Respectfully submitted,

W. G. STEVENSON,

Chairman.

The following gentlemen were elected officers of the section for 1887–1888:

Chairman, . . .	Mr. CHARLES N. ARNOLD.
Recording Secretary, .	Mr. JAMES WINNE.
Curator of the Museum,	Prof. WILLIAM B. DWIGHT.
Librarian, ...	Mr. CHARLES L. BRISTOL.

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE.

AND ITS

SCIENTIFIC SECTION.

POUGHKEEPSIE, N. Y.

1887-1890.

VOL. 5.

W. G. Stevenson

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

AND ITS

SCIENTIFIC SECTION.

1887-1890.

VOL. V.

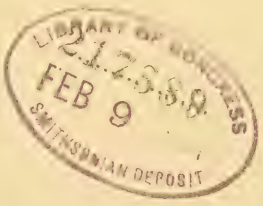
PART I.—Transactions of the Institute.

PART II.—Transactions of the Scientific Section.

PUBLISHING COMMITTEE:

EDWARD ELSWORTH, LEROY C. COOLEY,

JAMES WINNE.



POUGHKEEPSIE, N. Y.
PRESS OF A. V. HAIGHT,
12 LIBERTY ST.

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A. P. Van Gieson,
LeRoy C. Cooley,
William B. Dwight,
Charles B. Herrick,
Edward Elsworth,
Henry V. Pelton,
William T. Reynolds,
Stephen M. Buckingham,
Wm. G. Stevenson,
Charles N. Arnold,
Evan R. Williams.

1888–1889.

John Guy Vassar,
A. P. Van Gieson,
LeRoy C. Cooley,
William B. Dwight,
Charles B. Herrick,
Edward Elsworth,
Henry V. Pelton,
William T. Reynolds,
William G. Stevenson,
Charles N. Arnold,
Evan R. Williams,
Charles B. Warring.

1889–1890.

A. P. Van Gieson,
Le Roy C. Cooley,
William B. Dwight,
Charles B. Herrick,
Edward Elsworth,
Henry V. Pelton,
William T. Reynolds,
*William G. Stevenson,
Charles N. Arnold,
Evan R. Williams,
Charles B. Warring,
Irving Elting.

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*Died July 31st, 1890.

OFFICERS OF THE INSTITUTE.

1887–1888.

Rev. A. P. Van Gieson, D. D.,	*President.*
Mr. Henry V. Pelton,	*Vice-President.*
Mr. Charles L. Bristol,	*Secretary.*
Mr. Edward Elsworth,	*Treasurer.*
Prof. Henry Van Ingen,	*Art Director.*

1888–1889.

Rev. A. P. Van Gieson, D. D.,	*President.*
Mr. Henry V. Pelton,	*Vice-President.*
Mr. Charles L. Bristol, * Mr. James Winne,	*Secretary.*
Mr. Edward Elsworth,	*Treasurer.*
Prof. W. H. Cusack,	*Art Director.*

1889–1890.

Mr. Henry V. Pelton,	*President.*
Mr. Charles B. Herrick,	*Vice-President.*
Mr. James Winne,	*Secretary.*
Mr. Edward Elsworth,	*Treasurer.*

* Elected to fill vacancy—Dec. 11, 1888.

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

1887–1890.

NOVEMBER 15, 1887—FORTIETH REGULAR MEETING.

Twenty-five members, and two hundred and thirty guests present.

The president of the Institute, Rev. A. P. Van Gieson, D.D., opened the course of the season by delivering an address entitled: "Flags which have floated over American Soil," in which were presented a brief history of the national flags of Spain, the Netherlands, France, and England, and a more extended history of the flag of the United States, and of the successive flags of the Confederate States. The address was illustrated by colored representations of the various flags spoken of, and also showed very clearly how the chief points in American history were illustrated by the changes of the flags.

FEBRUARY 7, 1888—FORTY-FIRST REGULAR MEETING.

Rev. A. P. Van Gieson in the chair, twenty-three members, and two hundred guests present.

Rev. James M. Taylor, D.D., president of Vassar College gave an address entitled, "The Influence of Hume on Modern Thought," which was discussed by Dr. W. G. Stevenson, President Van Gieson, Rev. H. Loomis, Jr. and others.

MAY 2, 1888—SEVENTH ANNUAL MEETING.

Rev. A. P. Van Gieson, D. D., president, in the chair, and a quorum of members present.

The following named gentlemen having been previously proposed, were elected members: D. B. Ward, M.D., Frederick Arnold, W. H. Cusack, Rev. F. A. M. Brown.

The president made a written report of the work done during the year, as follows:

In compliance with the requirement of the by-laws the president submits the following report of the progress of the Institute and of the work done by its several sections during the past year.

The Institute has suffered grevious loss in the decease of Mr. Stephen M. Buckingham who was one of the orignal members of the Institute and of the first board of trustees, and in both capacities rendered faithful and valuable service until disabled by the sickness resulting in his decease.

Four new members have been elected. The present membership is one hundred and fourteen.

The finances are in a satisfactory condition, as will appear in detail from the report of the treasurer.

The following addresses have been delivered before the Institute:

1887.

November 15. "Flags that have floated over American Soil."
Rev. A. P. Van Gieson, president of the Institute.

1888.

February 7. "Influence of Hume on Modern Thought."
President J. M. Taylor of Vassar College.

SCIENTIFIC SECTION.

The following papers have been read before the Scientific Section:

1887.

December 6. "Woods and Gums." C. N. Arnold, Chairman of the Section.
December 20. "Miracle, Law and Evolution."....Dr. C. B. Warring.

1888.

January 10, (*a.*) "Some Practical Suggestions as to Preparation of Microscopic Sections of Fossils."
(*b.*) "The Exhibition and Description of New Devices in Machinery for making Sections of Minerals and Fossils."
Prof. W. B. Dwight.

January 24. "Relative Brilliancy of the Moon and Electric Light."
Dr. C. B. Warring.

February 14. "Recent Geological Study of this Vicinity."
Prof. W. B. Dwight.

March 27. "Notes on Amateur Photography."..... Prof. C. L. Bristol.

April 9. "Divisibility of Coloring Matter, and Sensibility of the Eye."
Prof. Leroy C. Cooley.

April 23. "Some Terrestrial and Astronomical Matters."
Dr. C. B. Warring.

The attendance at the meetings of this Section has been good, and great interest has been manifested in its proceedings.

LITERARY SECTION.

The Literary Section has held six public meetings in the auditorium on the evenings of January 17, January 31, February 21, March 20, April 3 and April 17, respectively. The first four meetings were devoted to a dicussion of the Land Theories of Henry George. On April 3 the Tariff question was discussed, and on the evening of April 17, Mr. E. Burgess presented a paper on the Relation of the State to Monopolies, which was discussed by the members present.

ART SECTION.

After a long period of enforced quiescence the Art Section has at last seen its way clear for active operation. The walls of the Studio have been suitably colored. Drawing tables, casts and other requisites have been supplied. Mr. W. H. Cusack has been appointed instructor and a school of art has been established. The school was opened for the reception of pupils January 4, 1888, and has ever since held regular sessions on the afternoons and evenings of Mondays, Wednesdays and

Fridays. The afternoon sessions are for women, and the evening session for men. The number of pupils is 70. The average attendance each day has been 41. The work done and the progress made by the pupils are creditable alike to them and their instructor. Not only the Art Section but also the Institute as a whole may well be congratulated on the success which at the very outset the Art School has attained. It is deemed proper to repeat here on behalf of the Institute the acknowledgment which has already been made by the Art Section, of the valuable aid rendered by Mrs. Abraham Wiltsie, in the procuring of the funds by which the opening of the Art School was made possible.

Respectfully submitted.

A. P. Van Gieson.

The treasurer, Mr. Edward Elsworth, submitted his report for the year ending May 1st, 1889, which report contained a detailed account of receipts and expenditures during that period. The following is an abstract:

Balance in Treasury May 3, 1887	$2,138 22
Total receipts during the year	2,346 04
Total	$4,484 27
Total disbursements during the year	2,051 30
Balance in treasury, May 1	$2,432 97

Prof. Cooley, chairman of the board of trustees gave an oral report of the condition of the property, which was accepted.

The following gentlemen were elected trustees of the Institute for 1888–1889:

John Guy Vassar,
A. P. Van Gieson,
Le Roy C. Cooley,
Wm. B. Dwight,
Charles B. Herrick,
Edward Elsworth,
Henry V. Pelton,
Wm. T. Reynolds,
Wm. G. Stevenson,
Charles N. Arnold,
Evan R. Williams,
Charles B. Warring.

The following gentlemen were elected officers of the Institute for 1888–1889 :

A. P. Van Gieson, ... *President.*
Mr. Henry V. Pelton, . . . *Vice-President.*
Prof. C. L. Bristol, ... *Secretary.*
Mr. Edward Elsworth, ... *Treasurer.*

The president appointed the following committee on Library and Museum :

Wm. G. Stevenson.
Le Roy C. Cooley.
Wm. B. Dwight.

The same committee was directed to take charge of publication.

TRANSACTIONS

OF

VASSAR BROTHERS INSTITUTE,

1888–1889.

DECEMBER 4, 1888—FORTY-THIRD REGULAR MEETING.

Six hundred members and guests present.

Rev. A. P. Van Gieson, D.D., president of the Institute, in opening the season of 1888–1889, paid a worthy tribute to the memory of John Guy Vassar, deceased.

The inaugural address "The Cathedral at Cologne," was illustrated, by the kind assistance of Prof. Le Roy C. Cooley.

No business meeting was held.

DECEMBER 11, 1888—FORTY-FOURTH REGULAR MEETING.

President Van Gieson and three hundred members and guests present.

Rev. E. A. Lawrence, of Sing Sing, N. Y., gave a most interesting address on "The Theosophists of India."

At the business meeting, a quorum of members being present, the President announced a vacancy in the office of Secretary. Upon a ballot being taken, Mr. James Winne was unanimously elected Secretary, vice C. L. Bristol, removed from the city. Dr. J. E. Hoffman and Mr. Gilbert Van Ingen were nominated for membership.

JANUARY 22, 1889—FORTY-FIFTH REGULAR MEETING.

President Van Gieson, and three hundred and fifty guests present.

Dr. J. Howard Suydam, of Jersey City, N. J., delivered a lecture entitled "How the Dutch took Holland." It was a succinct and graphic account of the visit of the Holland Society of New York to Holland during the summer of 1888.

At the business meeting, a quorum being present, Dr. J. E. Hoffman and Mr. Gilbert Van Ingen, were elected members of the Institute.

MARCH 5, 1889—FORTY-SIXTH REGULAR MEETING.

President Van Gieson, and two hundred and fifty guests present.

Prof. Henry Van Ingen, of Vassar College, delivered a lecture entitled "A Painting and its Component Parts," which was illustrated by stereopticon views.

No business meeting.

MAY 7, 1889—EIGHTH ANNUAL MEETING.

Rev. A. P. Van Gieson, D.D., president, in the chair, and a quorum of members present.

The president submitted his annual report as follows:

The president of Vassar Brothers Institute respectfully presents to the Institute the following annual report of the work done by the Institute and its several sections:

During the past year the Institute has suffered a grevious loss in the decease of Mr. John Guy Vassar, one of the two founders and liberal benefactors from whom the Institute derives its name. It is deemed proper that this annual report should contain not only a record of the fact of his decease, but also a renewed expression, in behalf of all the members of the Institute, of their respect for Mr. Vassar as a man, of their appreciation of his deep interest in the welfare of the Institute, of his liberality towards it during his life, and of the additional testimonials alike of his interest and his liberality which have come to light since his decease.

The record of the Institute in its general work for the past year is as follows :

1888.

December 4. Lecture on "The Cathedral of Cologne," by the president of the Institute, Rev. A. P. Van Gieson. No business meeting.

December 11. Lecture on "The Theosophists of India," by Rev. E. A. Lawrence, D. D.

The first regular business meeting of the year. James Winne was elected secretary, vice C. L. Bristol, removed from the city. Dr. J. E. Hoffman and Gilbert Van Ingen were proposed for membership.

1889.

January 22. Lecture on "How the Dutch took Holland," by Rev. J. Howard Suydam, D.D.

Second regular business meeting. Dr. J. E. Hoffman and Mr. Gilbert Van Ingen were unanimously elected members of the Institute.

1889.

March 5. Lecture on "A Painting and its Component Parts," by Prof. Henry Van Ingen.

The following is the record of the meetings held and the work done by the Scientific Section :

1888.

November 27. Informal meeting with no paper.

December 18. No paper. Election of Dr. C. B. Warring as chairman of Section, vice Mr. C. L. Bristol, removed from the city.

1889.

January 15. Prof. Wm. B. Dwight.
"Cambrian System of Strata."
"Discovery of Middle Cambrian."
"Strata at Stissing in Dutchess County, N. Y.

January 29. General discussion. No paper.

February 12. Dr. C. B. Warring.
"An Algebraic Conundrum."
"The Philosophy of Imaginary Quantities and their Graphic Representation."

February 26. Prof. Wm. B. Dwight.
"Glacial Phenomena."

March 12. General discussion. No paper.

1889.

March 26. Prof. Le Roy C. Cooley.
"Illustrations of Simple Harmonic Motion."

April 9. Mr. Edward Elsworth.
"The Development of Photography."

This being the annnal meeting the reports of librarian and curator were submitted and adopted by the Section.

The officers elected for the ensuing year are:

Chairman, ... EDWARD ELSWORTH.
Secretary, ... JAMES WINNE.
Curator, ... WM. G. STEVENSON.
Librarian, ... FRED. ARNOLD.

The Literary Section has held but two meetings during the year.

1889.

March 12. The subject of "Trusts" was presented by the chairman, Mr. Elsworth, and discussed by the members of the Section.

April 2. William C. Albro, Esq., spoke on "Electoral Reform," after which the subject was generally discussed.

Officers for 1889–90 have not yet been elected.

Three meetings of the Art Section have been held, viz: on September 25th, October 5th and October 9th, 1888. At these meetings arrangements were made for the continued maintenance of the Art School from October 15th, 1888 to July 1, 1889.

In accordance with these arrangements the school has been in regular operation under the direction of the art instructor, Mr. W. H. Cusack. Thirty-one pupils have been in attendance, and have made gratifying progress. By judicious expenditure of appropriations granted by the board of trustees, valuable additions have been made to the property under the care of this Section, in the shape of casts, drawings, books, and other requisites for efficient instruction.

Respectfully submitted,
A. P. VAN GIESON,
President.

The treasurer, Mr. Edward Elsworth submitted his report for the year ending May 7, 1889, which report contained a detailed account of receipts and expenditures during that period.

Balance in treasury May 1, 1888	$2,432 97
Total receipts during the year	1,954 30
Total	$4,387 27
Total disbursements during the year	1,967 25
Balance in treasury May 7, 1889	$2,420 02

Prof. LeRoy C. Cooley, chairman of the board of trustees, submitted the following annual report:

Vassar Brothers Institute: The board of trustees, complying with a requirement of the by-laws of the Institute, beg leave to make the following statements in regard to the condition of the property of the Institute.

All necessary repairs have been made during the year and the building and grounds are in good order.

Large additions have been made to the Library by the receipt of all our current exchanges and by the purchase of useful and standard scientific works. A careful index of the entire library is being made which, by facilitating research, will greatly increase the value of the collection.

The museum has been enriched by the addition of a large number of specimens. The herbarium which, hitherto, has been in much confusion has been carefully examined, its specimens identified and properly labelled and the whole collection put into good working order.

Two new wall cases have been provided for the display and protection of the herbarium: the floor cases have been repainted and the room otherwise improved.

The report of the treasurer will show the good condition of the Institute funds.

Very respectfully reported,

LE ROY C. COOLEY,

POUGHKEEPSIE, May 7, 1889. *Chairman.*

The following gentlemen were elected trustees for 1889–1890:

Charles N. Arnold,	Henry V. Pelton,
LeRoy C. Cooley,	William T. Reynolds,
William B. Dwight,	Wm. G. Stevenson,
Edward Elsworth,	A. P. Van Gieson,
Irving Elting,	Evan R. Williams,
Charles B. Herrick,	Charles B. Warring.

The following gentlemen were elected officers of the Institute for 1889–1890.

President, . . .	Henry V. Pelton.
Vice-President, . . .	Charles B. Herrick.
Treasurer, . . .	Edward Elsworth.
Secretary, . . .	James Winne.

TRANSACTIONS
OF
VASSAR BROTHERS INSTITUTE.
1889–1890.

NOVEMBER 12, 1889—FORTY-EIGHTH REGULAR MEETING.

The president Mr. Henry V. Pelton, read a paper, entitled—"Literary Association," as follows—

One of the most familiar ideas at the present time is that of association. It is not peculiar to this age but it has recently become greatly developed. There seems to be no limits to its growth. Manifold are the circumstances which call it forth; innumerable are the purposes for which it is put into operation. It has passed beyond the point where its advantageousness may be discussed. To dispute it would be to quarrel with the experience of civilized man in all places and at all times.

The earliest organizations were, doubtless, those relating to trade, as the earliest ideas developed in man, after those of self-preservation, were those which sought the improvement of his condition and circumstances, through barter with his fellow-man. It was much later before associations were formed within the limits of professional practice or among those whose labors were in the higher branches of the arts. But both of these, in most cases, if not all, antedated those organizations which brought together those who followed divers callings, where the tie which held them was not the similarity of their occupations but the agreement of their tastes. Now that these three classes are in full tide of both operation and growth, my statement, made a moment ago, that we could set no limits to the growth of this idea of associ-

ation, is no exaggeration. The practice extends regardless of all distinctions. No rank in life is so high, no riches are so abundant that their possessors are, on that account, free from connection with voluntary associations; and if we look among those whose lives are spent in the lowest degradation and poverty, we shall find, in some order or other, organization. All sorts and conditions of men, and associations distinctly belonging to each sort and condition. Richman, poorman, beggarman, thief, each and all find the club door open to them.

When we look at the objects which are sought in organization, manifestly these divide into two classes, the unselfish and the selfish. The first include the genuinely religious and charitable organizations and the second, all others. In these others are sought the advantage of the members themselves. The business associations strive by consultation and united action to improve methods, develop capabilities and remove obstacles, that increased profits may accrue; the artist and professional men expect to find in association both incentive to study and advice and assistance in the prosecution of their work, and the others, one and all, have associated themselves together because they saw or thought they saw, that they themselves should reap benefits therefrom. Of course the benefit may not be, perhaps rarely is, pecuniary. It may lie wholly in the satisfaction which either the pleasure of meeting together, or the improvement which is the result of such meeting, yields them. Even if the increased efficiency, which is sought, is to be afterwards used for the good of others, none the less is it true that, in the first place, a personal advantage is desired.

Associations among writers were not common in past times. At a period when guilds were organized in many callings for mutual aid and defense, writers dwelt and worked apart. And this has been changed, even in our

own time, only in a limited degree. Compared with other callings, the associations among authors have been less formal and less influential. There is nothing to-day in English or American library circles, corresponding to the Academys' in Art with their diplomas. It is doubtless true that the nature of their work is such that association could not help these to the same extent as the others, but it is also true and has been a more potent cause, that in all ages, writers have been noted for their quarrels, and only occasionally could their jealousy and sensitiveness brook any but the most formal intercourse with those who were rivals, and possibly, also, critics.

But literary association is, of course, not confined to authors' clubs. While it is not easy to define the word literary, when used as a designation for associations it clearly has a wide significance and includes those gatherings where either the subjects considered or the interests which holds them together relate either to literary or to economic, social or political topics.

But it is of literary associations in the concrete rather than in the abstract that I wish principally to speak. To look at the subject historically instead of critically. To present some facts relating to literary clubs, circles, and coteries of the past and present time.

As we glance at some of these organizations we shall of course find that their literary character did not preclude other characteristics. Often literature held no undivided sway. There was high thinking but it was not thought necessary there should also be plain living. The convivial and the literary combine frequently and easily. It has been said that all celebrated clubs were founded on eating and drinking. Indeed the word club is, in its deriviation, allied to our word cleave, and includes the meaning both to adhere and to divide, and had its significance in the division of the reckoning among the guests around the table.

When we look at the Clubs of the past, we become doubtful whether the spirit of association is a characteristic of this age or of man *per se*. There is no scarcity of them in the times of which we have complete records and there are indications and rumors of them much farther back.

Points of similarity have been pointed out between the modern club and certain organizations of ancient Greece and Rome. There were public tables in Sparta whose members were elected by ballot and were subject to rules. The Athenians had friendly meetings where everyone sent his own portion of the feast. A club in early Rome is said to be spoken of and Cicero tells of the pleasure he took in the meetings of social parties called confraternities, the principal satisfaction to him arising "less from the pleasures of the palate than from the opportunity afforded of enjoying excellent company and conversation." And one writer has discovered the club, full-fledged, in the heart of the Dark Continent, for he says — "In the centre of almost every village in equatorial Africa, there is a large, barn-shaped building, in which laws are enacted, criminals tried, &c., but which also is used as a club-house, where, immediately after the morning meal elders of the tribe resort to pass the heat of the day, smoking or gossiping or listening to singing. Here no woman is permitted to enter."

In France literary associations flourished long ago. Charlemagne founded an Academy in which every member took the name of some great poet, the King himself being King David. The present French Academy had it origin in a literary club consisting of several inferior poets, who met early in the seventeenth century, at the house of one of their number. As this grew, Cardinal Richelieu, wishing thus to get a hold over literature, in the interest of the Court, founded for them the

Academy. For fifteen years afterwards it had no fixed place of meeting.

In the seventeenth century poets met regularly at taverns in Paris and, in some cases, ladies of rank formed literary societies which met at their houses. Such a society called La Parvoise, met every evening at the house of Madame de Persan, and those who came sat each in his own chair under his portrait. Madame de Persan's *valet de chambre* acted as secretary and sat in the middle of the room, before two large registers. The visitors then informed the hostess where they had been that day what they had seen and what they had heard. The news was discussed, divided into two classes and then entered into the registers, one of which was labeled "Doubtful facts" and the other "Proved facts." At the end of a week, extracts were made by the secretary and written on fly sheets, which he sold for his own profit. This was the origin of the "*Nouvelle-a-la-main*," a journal which continued until the time of the Revolution.

The most noted tavern club of early times in France was called "La Caveau" from the name of the tavern in which they met. It lasted eighteen years and the "New Caveau" continued until 1827. The members were all literary rivals yet we are assured that, not only did they dine in harmony, but after dinner they read to each other, for criticism and suggestion, their newly written operas, verses and songs without giving or taking offence. If a member wished to be aided in a new work, a day was appointed for reading it and the club assembled and gave the work the most careful scrutiny and the plainest criticism.

But the most famous spots of literary association in France have been the Salons. The Salon is peculiarly a French institution, and though in other countries, imitations have been attempted, nowhere else has it flourished.

In one respect it presents a marked contrast to the club, for while the club is essentially masculine, the Salon is wholly under female influence and guidance.

One of the earliest was held about the middle of the seventeenth century by Mme. da Rambouillet, a Florentine by birth, who brought with her to Paris a love of poetry, and there gathered around her all the lovers of literature. During Napoleon's reign the Salon flourished and Mmes. Le Brun, de Stael, Recamier, de Scudery, are names famous in the development of this institution and influential in deciding the fortunes of the nation. The Salons of Paris were Napoleon's chief opponents.

As we turn to England, we are impressed more than before with the antiquity of the habit of organization. Addison, in one of the Spectators, referring to man as a social animal, says, "we take all occasions and pretences of forming ourselves into those little nocturnal assemblies which are commonly known by the name of clubs. When a set of men find themselves agree in any particular, though never so trivial, they establish themselves into a kind of fraternity."

A club is said to have existed in the time of Henry IV and that to it Chaucer probably belonged and there are also reports that Sir Walter Raleigh established, about 1603, at the Mermaid tavern, the Mermaid Club, of which Shakespeare, Beaumont and Fletcher were members, but these stories are now doubted.

Early in the seventeenth century, Ben Jonson was a leading spirit in the Apollo Club which met at the Devils' tavern, and Pepys speaks of going to the Coffee House where had been founded a club called "Rota," in 1659, as a debating society for the dissemination of republican opinions.

Nearly all the early clubs and most of them until quite recent times met at the taverns. This, partly because

the tavern was the usual place of assembly on all occasions, and was the place for gossip and for news before the newspaper had developed so it could reach the people; but a still stronger reason was because, to an Englishman (and in this respect, I think, we have inherited from him) a gathering for any purpose is incomplete without a dinner. Douglass Jerold has said, "If an earthquake were to engulf England to-morrow, the English would manage to meet and dine somewhere among the rubbish, just to celebrate the event."

The "Kit-Kat Club," while partly political, was literary and artistic as well. It was a society of Whig leaders, consisting of thirty-nine noblemen and gentlemen attached to the House of Hanover. It first met at an obscure house in Shire lane kept by Christopher Katt, a noted mutton-pie-man, Chris, being abbreviated into Kit.

One of the most noted of the early clubs was the "Beef-Steak Society," founded by Rich, the first harlequin; or which might more correctly be said to have grown from Rich's habit of himself broiling a beef-steak for his dinner, in the room in Covent Garden Theatre where he arranged the tricks for his pantomime. Here men of rank and wit resorted and finally stayed and shared his steak. From this grew the "Saturday's Club" which met for some time in a room in the playhouse and afterwards at the Shakespeare tavern. The bill of fare was restricted to beef-steak and the beverage to port wine and punch. Both Hogarth and Garrick were among the earlier members. When the gridiron, upon which Rich had broiled his solitary steak became insufficient for the guests, it was enshrined as one of the household emblems of the club. This gridiron was found among the ruins after the Covent Garden fire. Through all the early years of the club it was customary to register in the weekly records anything of striking

excellence that had been said in the course of the evening, but these records perished in the fire. Fifty years later, when the Prince of Wales, Duke of Norfolk and the poet Morris were of the members, we find this description of a meeting. "The Duke took the chair, which was elevated some steps above the table and decorated with the insignia of the society. As the clock struck five, a curtain drew up, discovering the kitchen, in which the cooks were seen at work, through a sort of grating, upon which was this inscription, 'If it were done when 'tis done then 'twere well it were done quickly.'"

There have been, at times, associations of congenial spirits, whose meetings were, at first, unpremeditated, but which, becoming habitual, came to answer the same purpose and exert the same influence, as the formally organized clubs. Such a literary coterie held together for many years in the Chapter Coffee House, a noted resort for men of letters. The box in the north-east corner came to be known as the Wittenagemott. Here used to gather daily, a knot of editors, writers and book sellers, designated as the "Wet Paper Club," as it was their practice to open the papers when brought in by the newsmen and read them before they were dried by the waiter.

In recalling the literary circles of the past, the minds of all of us, I am sure, would very early turn to those houses which became celebrated, not only for hospitality given both generously and gracefully, but still more for the intellectual character of the gatherings; while elsewhere fashion and gayety ruled, in these lofty purpose and high thinking held sway. Such a one was Gore House, where, about the middle of this century, Lady Blessington maintained soirees, bringing together people of the same pursuits, who were rivals for professional distinction; her aim being to bring about harmony and

co-operation among literary men. But gatherings far more notable and more influential to the same end, were those of Holland House, where through many successive years, under the skillful guidance of Lord and Lady Holland, and dominated by the spirit of Christian charity which controlled both of them, there met a succession of cliques, comprising nearly every one eminent in letters or statemanship ; in art or the drama. To give the names of the guests is almost to form a list of those names distinguished during those years.

It seems a considerable descent from these gorgeous entertainments, where the leaders of the English world used to meet, to turn from them to those Wednesday nights when Charles and Mary Lamb received their friends in their humble home, but one who knew both places familiarly, Sir Thomas Noon Talfourd, has not hestitated to compare them, saying "the conversation which animated each of these memorable circles approximated in essence much more nearly than might be surmised from the difference in station of the principal talkers." At each, literature and art supplied the favorite topics.

Montagu House also deserves mention among the spots made famous by the gatherings of the learned and studious. Having imbibed, when young, her taste for learning, through her friendship with many scholarly men, Mrs. Montagu received in London an assemblage of intellectual persons. At first this was done unpremeditatively, but afterwards with the design of drawing such persons into fellowship and making a centre for literary society in London. At these assemblies she brought together a distinguished company, made up of authors, actors, divines and agreeable women. Here came Erskine, Beauclerk, Garrick, Lord Lytellton, Walpole, Lord Chatham. "No Englishwoman," it is said, "ever succeeded so completely in drawing men from the club

and women from the dance and the gaming table to the disquisition of literature and science, as Mrs. Montagu." These assemblies continued in their perfection for fifteen years, until 1785. It was to these assemblies that the term "blue stockings" was first applied. It originated from a remark made by Admiral Boscawen. One of those who frequently attended was Dr. Stillingfleet. He was a learned divine and admirable in conversation, but was somewhat of an oddity and sloven, and appeared in blue stockings. When absent he was so missed that it was said "we can do nothing without the blue stockings." From this the name "Blue Stocking Society" was applied to these meetings, indicating that the full dress, then universally worn in society in the evening, was to be dispensed with. Hannah More has celebrated these gatherings in her poem entitled "Bas-bleu or Conversation," depicting in their particular roles, those who especially shone there by their wit or learning, and distinguishing between the spirts which severally ruled here and in the equally noted Salons of Paris, and giving to the charms of conversation no stinted praise.

Probably there is no part of the history of literary clubs which is so universally interesting as that which centres about the person of Samuel Johnson. It was he who coined the word "clubable" and few if any have so exemplified it in their own persons. He has defined a club as "an assembly of good fellows, meeting under certain conditions," and in the different clubs of which he was the instigator or founder, those conditions always were that serious and thoughtful conversation should rule. These clubs were his chief delight, and in them this hard worker found the relaxation and recreation which lifted him above the bread-winning drudgery which was for a long time his lot.

Macaulay says of Johnson's conversational power, "it was nowhere so brilliant and striking as when he was surrounded by a few friends, whose abilities and knowledge enabled them, as he once expressed it, to send him back every ball that he threw."

While at work upon his dictionary, Johnson first formed a club, called the Ivy Lane Club, meeting at the King's Head in Ivy Lane. It had but nine members and lasted until 1756. In his latter years, in 1781, a small club was organized which met at the Queen's Arms in St. Paul's Churchyard, and two years later Sir John Hawkins, gives an account of a dinner which resulted in the formation of another. At this dinner met the four survivors of the Ivy Lane Club formed thirty-six years before. "At ten," says Hawkins, "we broke up, much to the regret of Johnson, who proposed staying, but finding us inclined to separate, he left us, with a sigh that seemed to come from his heart, lamenting that he was retiring to solitude and cheerless meditation." This brings vividly before us the loneliness of his old age. It was to insure himself society in the evening that at this time he instituted the Essex Club, which lasted until his death.

I have left until the last, that best known and most successful of Johnson's clubs, though it was founded in 1764, long before those just spoken of. It grew out of casual but frequent meetings at the house of Sir Joshua Reynolds and met Mondays of each week, for some years, at the Turk's Head in Gerrard Street. For fifteen years it existed without a name, but after Garrick's death it became known as the "Literary Club." It's original members besides Reynolds and Johnson, were Edmund Burke, Dr. Nugent, Mr. Beauclerc, Mr. Langton, Dr. Goldsmith, Mr. Chanier and Sir John Hawkins. Here, Boswell tells us, Johnson spent his happiest hours and never absented himself from the meetings.

The talk was miscellaneous but chiefly literary, politics alone being excluded. At the meetings, the chair was taken in rotation by the members, selected apphabetically, the only permanent officer being the treasurer. The membership of the club was gradually increased until it reached thirty-five. The proposition to enlarge the club came from Goldsmith. "It would give," he thought, "an agreeable variety to our meetings, for there can be nothing new among us, we have travelled over each other's minds." This remark piqued Johnson, who replied "Sir, you have not travelled over my mind, I promise you."

Mr. Fosters says of the club. "The fame of it's conversation received eager addition from the difficulty of obtaining admission to it, and it came to be generally understood that literature had fixed her headquarters here, with advantage to the dignity and worldly consideration of men of letters." Said the Bishop of St. Asaph; "The honor of being elected into the Turk's Head Club is not inferior to that of being the representative of Westminster or Surrey." The Bishop had just been elected but even thus early (it was but fifteen years old) Bishops and even Lord Chancellors were known to have sought admission unsuccessfully.

The character of the club has changed somewhat in its later years. In the first fifty years of its existence, three-quarters of its members were authors, now it has very few except titled members, but the majority of these have some claim to literary distinction and the club still acknowledges the love of literature as the tie which holds its members together. It's long list of members, since its foundation show very many names distinguished in letters, in government, in science, in art and in each of the professions. The name of the Society has been changed to the "Johnson Club." In 1864 it celebrated its centenary.

It is not probably that any other like organization, has ever existed, equally distinguished and influential. Speaking of its earliier days, Macaulay says "Its verdicts on new books, speedily known, were sufficient to sell off a whole edition in a day or to condemn the sheets to the service of the trunk-maker and pastry cook.

In 1824, some young men met regularly at a tavern in the neighborhood of Covent Garden Theatre. It was a regulation of this organization, that some paper or poem or conceit bearing upon Shakespeare should be contributed by each member. Later Douglas Jerrold and Laman Blanchard became members, and of course the entrance of two such as these, added no inconsiderable interest to the club. Upon Jerrold's suggestion, the club was called "The Mulberries," and their contributions were called "Mulberry Leaves." The club lived many years and included both actors and artists as well as writers, among its members.

The crop of leaves contributed by the members are still existing, and were to have been published, but no one would undertake to see them through the press. Afterwards the club was much changed in character and was called the "Shakespeare Club," having Charles Dickens, Talfourd, Maclise, and Macready as members. Jerrold afterwards belonged to several clubs; "The Hooks and Eyes," "Our Club," and others, and in each of these he was *the* member.

It was in these gatherings that many of those bright sallies of wit, in which he so excelled all others, were uttered, thus:

At a dinner of artists, a barrister present, having his health drank in connection with the law, began his response by saying he did not see how the law could be considered one of the arts, when Jerrold helped him out by ejaculating the word, black.

A supper of sheeps-heads was served, and one of the

gentlemen was particularly enthusiastic upon the excellence of the dish and exclaimed, as he threw down his knife and fork, "well! sheeps-heads forever, say I." "There's egotism," remarks Douglas Jerrold.

Very recently, one of our prominent magazines published an article describing London mock Parliaments, and I venture to give a few brief extracts from that article, feeling that though it must have been read by many of my audience, I should else pass over most important facts relating to my subject.

The oldest of the bodies there described is "The Temple Discussion Forum" which now meets every evening in the "Green Dragon." It was established in 1667. Across the window every morning is placed a paper announcing the subject of the evening's debate, and inviting strangers to enter and engage in the discussion. Says the writer "For more than two centuries, generations of incipient statesmen, diplomatists, and Queen's counsel have made their maiden speeches here, and these walls have echoed to the budding eloquence of many a future Lord Chancellor." The chairman sits at the top of the room upon a mahogany and horse-hair throne. Many of the frequenters were elderly men who looked as if they had passed their whole lives here. The eloquence was rough, the argument was vigorous, and the convictions evidently strong. Ale, porter and spirits were served.

Ye ancient Society of Cogers was established in 1755. Here, also, the question for each evening is posted, and strangers invited. On the walls of their room hang numerous portraits of distinguished Cogers of past generations. Nearly everybody drinks stout and smokes a pipe, and long-stemmed pipes are scattered about on all the tables, free to all. The men who frequent the meetings are of all ages, classes and conditions; budding lawyers, newspaper reporters, clerks, artisans and tradesmen. The discussions are always well disciplined and good hu-

mored. Formerly they were not limited to politics but covered a wide range. The society was especially fond of mysterious murder trials and great law cases, which they re-tried in all phases, sometimes occupying thus several successive evenings, and the verdict, taken by show of hands, often differed from that of the courts. Within half a dozen years, however, politics has driven all other subjects out of the arena.

Other societies of the same sort are described, but we are also told of the establishment, in various English cities, of societies of a different character. These are veritable mock Parliaments. The first was formed at Liverpool, but they also exist in Glasgow, Bristol, Manchester and other cities, and in London are half a dozen. These are debating societies, modelled almost precisely after the form of the House of Commons, with Speaker, Premier, Chancellor of the Exchequer and other ministers, leaders of the opposition, whips, &c., having rules of procedure and printed bills. The members are principally of the leisure class and members of Parliament, barristers and professional men, with a sprinkling of the aristocracy. Debates are upon the same subjects that are being nightly argued in the House of Commons.

The existence of these societies brings prominently before us one marked contrast between our own country and England. Here few deliberately enter upon a public career with the idea of obtaining honorable distinction therein. To start out with definite, acknowledged purpose to seek fame in public office is, among us, I am afraid, regarded as unworthy of one of high principle. But, as we all know, every Englishman regards it as an honorable ambition not only to seek for public office, but likewise to strive to fit himself to attain eminence and success therein.

Of literary associations in our own country, I have much less to say. That they exist to-day in large num-

bers, in all parts of the country, there is no doubt; but both because nearly all of them have been formed in very recent years and because little has been published concerning them, interesting facts relating to them are not accessible. There is, I believe, in Philadelphia, a club of this character, which has existed more than one hundred years, but I know of no other that has attained even so moderate an old age.

Among the knot of authors and students who made Boston the literary centre a quarter of a century ago or a little more, there were formed, probably, the most notable of any American associations of this character. The name about which, in most minds, the thought of these Boston gatherings centre, is that of Emerson. Yet, though he was a member of many of them, he was, to use again Dr. Johnson's word, in some respects one of the least "clubable" of men. His biographer tells us that he was always ready for a project of a club or meeting for conversation, though all his experience was against them.

"Shall I paint," says Emerson in his journal, "an experience so conspicuous to me and so often repeated in these late years as the Debating Club, now under the name of Teachers' meetings, now a Conference, now an Esthetic Club and now a Religious Association, but always bearing for me the same fruit, a place where my memory works more than my wit and so I come away with compunction."

Emerson was one of the original members of the "Saturday Club" which included also Longfellow, Holmes, Agassiz, Judge Hoar and Lowell. Indeed Holmes says that Emerson was the nucleus around which the club formed itself. He enjoyed the meetings and went regularly until his powers began to fail, but, while he extolled the conversational powers of his associates, his own attitude was that of a listener. Carlyle

says "he seemed on such occasions to come with a rake to gather in and not with a shovel to scatter abroad."

Mr. Cabot says, this reticence was caused, to a large degree, by his nicety in the use of language. "To give the thought just and full expression, he must not prematurely utter it."

The increase of associations of this kind throughout this country is remarkable and without doubt, is significant. From the great centres and capitals they have spread to every city not only, but to many villages. Everywhere, in some form or other do we find the literary club, or association, or circle, with its written paper and its verbal discussion. Of course in the large cities they are multiplied and classified. New York has a Greek club at whose meetings nothing but Greek is spoken. The "Authors Club" of New York is an organization of importance enough to maintain permanent club rooms. Its aims are more social than literary, but it designs to bring the older men of letters into more intimate relations with the younger. No person is eligible who is not an author of a published work, proper to literature, or who has not a recognized position in other kinds of literary work.

Evidently the verdict, growing ever more and more unanimous, is in favor of literary associations. There is no need of argument in their behalf. But the danger, that Emerson expressed, of premature utterance, is no imaginary one nor does it fail of frequent illustration.

"The American mind" he says, "is too demonstrative; most persons are over expressed, beaten out thin, all surface, without depth or substance;" and he recommended a rule for such meetings that no one should reply to what had been said by another speaker.

But the advantages clearly outweigh the dangers. How much delight and inspiration have even the strongest and brightest minds found in the talks which

such associations have produced. Even Thoreau, who loved solitude so much that he improvised a hermit's cell, came out once and again to meet and talk with Emerson. No mind is sufficient unto itself. In the contact of the literary circle, in its interchange of ideas, its questioning of opinions, its antagonizing of convictions, individual tendencies and peculiarities are harmonized and improved.

Conversation and discussion also awaken thought. With most people thoughts upon any subject are uncertain and indefinite until some outward expression is made. Thus in talking we find out what we think and what we know or do not know, and sometimes the last is quite a revelation to us of our ignorance upon subjects on which we had considered ourselves informed.

Another and by no means the least important result from literary association has been increase of information. Especially is this true, as relates to all political, social and economic questions. "The clubs of England and the Salons of France" remarks Madame Mohl, "have long been places where, like the porticoes of Athens, public affairs have been discussed and public men criticized," and like organizations here have greatly aided in disseminating among the people, just views of government and of their own political rights and duties.

It is a part of the work which ought not to be neglected. A prominent judge has recently recommended "the establishment by societies, of volunteer legislatures and congresses to meet and discuss and act upon political and economic propositions presented in the shape of bills and carried through regular stages."

We have taken a wide range, in time and distance, in our review of literary associations, but we may at least end at home, with a word in reference to the organization in whose name we are met to-night. More than twenty-years ago the "Young Men's Literary Union" was or-

ganized in this city and under that name and the one subsequently adopted, "The Poughkeepsie Literary Club," the work was continued until the founding of this institute through the munificence of Mr. Vassar, when it was merged, together with the younger sister of scientific aims, in this splendidly endowed institution.

For more than twenty years, at least, literary association has been a present influence in this community. We have no means of determining what it has accomplished, but no one, who has watched it through these many years of activity, will doubt that its power has been both potent and beneficial. It is but a part of our work in this institute, but it is a part which, with the abundant facilities and opportunities here afforded, ought to be prosecuted with energy and with enthusiasm.

At the business meeting the following resolution with reference to Article II, § 7 and § 8 of the by-laws was adopted:

"That the annual dues of Vassar Brothers Institute "be made one dollar, and the fee for initiation two "dollars."

Mr. Charles B. Warring nominated for membership Mr. J. B. T. Tuthill.

Mr. Edward Elsworth nominated Mr. George B. Rogers. Action with reference to a classification of trustees was deferred till a subsequent meeting.

DECEMBER 3, 1889—FORTY-NINTH REGULAR MEETING.

In the absence of president Henry V. Pelton, Mr. Charles B. Herrick, vice-president, introduced to the Institute and about one hundred invited guests, Dr. William A. Mowry, of Boston, who delivered a lecture on the "Discovery, Exploration and Settlement of the Northwest Territory (Oregon), by the United States."

Following is a brief extract thereof:

The English say we have no history. One English lady asked an American if there were any large trees in America, and half apologized for asking so absurd a question by remarking, "of course not, for it is only a new country." We can't match them in historical dates; but we make more history in ten years than England in a century. Methusaleh, when five hundred years old, was asked by an angel why he did not build and live in a house instead of a tent. The old man asked the angel if he supposed five hundred years more of life would be given him. The angel could not say. Then, said Methusaleh, it won't pay. Why, in a single decade, has more history been made than the whole time of Methusaleh? In ten years from '60 to '70, we liberated 5,000,000 slaves; Russia 30,000,000; and Brazil 10,000,000 more.

In 1833 St. Louis was the outer post of civilization. There a delegation of Nez Perces came to Gen. Clark asking that there be sent to their tribe "that Book with men near to God, to teach the right way to live." Gen. Clark, who was a Roman, entertained them with the sights and pleasures of the station; but they had nowhere been shown the book. At parting the old man of the delegation rebuked the general for his neglect, and declared the visit a failure. The words of the old man were taken down by a clerk of the agency, and were borne to the east and caught up by a missionary organization. Extraordinary results followed.

Marcus Whitman, a physician, with Parker, Eals and Spaulding, were sent to the Nez Perces, established missionary stations in the upper valley of the Columbia at Walla Walla, Idaho and Spokane Falls, and commenced their labors.

The political history of the region begins with no owner for the Oregon region and for a long time it was known as "No Man's Land." Spain was supposed to

hold some claim to it; but we always claimed it. In 1792 a Boston Yankee, Captain Gray, attempted to enter Columbia River, but failed, and sailed away; he met Van Couver's vessel off shore, and informed that worthy of his belief of the existence of the large river. Van Couver scouted the idea, and insisted that no such river existed. Captain Gray at once returned to the mouth of the river, with difficulty passed its bar, and sailed up the stream, planted a flag, and claimed the country by right of discovery. Jefferson, before acquiring the northwestern territory, projected a survey west of the Missouri, and Lewis and Clark started in 1804 to make it, and in 1805–6 the government held the region. In 1810 Astor, with two detachments, proceeded to the region and established trading posts on the Columbia with outposts extending to the Missouri. The history of "Astoria" is full of the most interesting personal and political incidents. Astor had taken English, Scotch and Canadian partners, who became traitors to Astor but were loyal to Great Britain, and sold Astor out, and raised the English flag over the fort there. In the then war a Captain Black was sent from England to capture this Yankee fort, and after a nine month's voyage to batter it down, was surprised to see the cross of St. George flying, and to be hailed with "what's the news from 'ome?" The farce of raising the American flag and pulling it down was played, and the region was listed as among "all places captured during war."

We never, before 1819, claimed full title through Spain's treaty with us. We gave up Texas, and Spain gave up Oregon. The Hudson Bay Company dotted the district with stations in 1842. A missionary letter, starting three hundred miles from the Pacific Coast, taken by a whaler to Rhode Island, thence to Boston, gave warning of the designs of the Hudson Bay company. Whitman called the missions to meet at Walla

Walla, and asked permission to go east and tell the people of the value of the country, and ask aid to colonize it, against the Hudson Bay Company. The meeting objected to his scheme as political, and urged him to remain and devote himself to missionary work. He replied "I was a man before I became a missionary ; I did not lose my manhood when I became one." He left Walla Walla October 3d. His journey was a marvel. A lad was his companion with a guide. Lost in the mountains, swimming icy rivers, reaching the frontiers, gathering there recruits for his settlement, who were to meet him on his return, he went to Washington, enlisted the interest of President Tyler, Daniel Webster, and officers of the government, then visited Boston, where the society agreed to everything Whitman asked.

Whitman returned with eight hundred emigrants with wagons and fifteen hundred head of cattle, who dispersed themselves in the valley of the Willamette, the garden of the earth. Here are grown all the tropical products. The speaker had never elsewhere seen wheat produce a hundred fold, but had seen forty fold. Here a single grain spouts one hundred and twenty-three stalks with one hundred full heads on them, or four thousand fold. Thus the country was taken possession of.

In 1842 at Walla Walla was held what was called the "Wolf meeting" to devise ways and means for protecting stock. A Yankee present suggested protection of themselves against the Hudson Bay Company, and proposed action to that end. A vote was taken and one majority for the Yankee idea given. When Whitman came back with his eight hundred followers, the Yankee triumphed, with the result that the 4th parallel was made the northern boundary.

At the completion of the address a vote of thanks to Dr. Mowry was moved by Dr. W. G. Stevenson, and unanimously passed.

No business meeting was held.

FEBRUARY 4, 1890—FIFTEENTH REGULAR MEETING.

President Henry V. Pelton introduced to the members, and about seventy guests, Rev. Geo. D. Hulst, state entomologist of New Jersey, who discussed "Insects and their Habits."

At the business meeting which followed, Mr. J. B. T. Tuthill and Mr. George B. Rogers were unanimously elected members of the Institute.

The president appointed as a committee on publication,

Dr. Wm. G. Stevenson, Edward Elsworth and James Winne.

Committee on Museum and Library,

Dr. W. G. Stevenson, William B. Dwight and Charles N. Arnold.

Dr. Stevenson nominated for membership Wm. Silas Wodell.

MARCH 4, 1890—FIFTY-FIRST REGULAR MEETING.

President H. V. Pelton introduced to a large audience Prof. W. LeConte Stevens, of Packer Collegiate Institute, Brooklyn, N. Y., who gave a most interesting lecture on "Sensitive Flames and Sound Shadows." The lecture was accompanied by lantern illustrations, and a series of most delicate experiments in Acoustics. The most recent discoveries in the diffraction of sound, which have been given to the public only by Lord Raleigh at the Royal Institute in London, and by Prof. Stevens in this country, were most successfully presented to the audience.

At the business meeting Mr. Winne nominated for membership Dr. Theodore Neumann.

APPIL 1, 1890—FIFTY-SECOND REGULAR MEETING.

President Henry V. Pelton and about three hundred and fifty members and guests present.

Prof. Charles A. Young, of Princeton College, N. J., delivered a lecture entitled, "Nebulæ and the Nebular Hypothesis," which was fully illustrated by lantern views.

Following is an abstract of the lecture:

To the comprehension of the speaker, the orderly development of the planets is to the scientific mind preferable to their instant creation, as a theory, and brings men nearer to the creator. The law of progressive growth, the evidence of the operation of forces, confirmatory of the germ theory, presents fewest difficulties to minds seeking to understand the plans which nature and the world about us present. Theorists differ in essentials, and their differences cannot be reconciled. The spacings of planets, their distance from each other, accounted for by one, and which are in harmony with one of the most acceptable theories, are not accounted for, and even not noticed, in others.

The word nebula means simply a cloud, and in astronomy is used to denote objects which, to the eye, either unaided or through a telescope, appear to have a similar texture, shining with a faint, hazy light, like that of the milky way, or of a thin cloud illuminated by the sun. Objects of this sort are numerous in the heavens. Within the solar system we have that magnificent glory of the sun's corona, the comets, with thin, streaming trains and the mysterious zodiacal light. In the region of the stars there are comets by the thousand, some few of them bright enough to be seen with the naked eye, but for the most part so faint as to be observed only with the great telescopes. As to the nature of this nebulous matter and the cause of its shining, there is much as yet uncertain. It is quite probable that different nebulosities differ in their constitutions and nature. Some are, perhaps, mainly composed of matter in the form of gas, with little admixture of solid or liquid particles. More

usually, however, there is reason to suppose that they are constituted like clouds on the earth, that is, composed of particles, solid or liquid, surrounded and interpenetrated by gaseous matter, such as a cloud of water, rain, fog, snow, smoke or dust. The nebular hypothesis is a name given to an explanation of astronomical evolution, according to which explanation the suns and planets, as we observe them, have come to their present condition by the gradual condensation and aggregation of nebulous matter. While much doubt remains as to the many details of the process, and although there seem to be some difficulties as yet insuperable in the way of all different and rival forms the history has taken, as expounded by different authorities, it seems almost certain that in its main features it is correct. Swedenborg, in 1734, seems to have been the first to express the main idea, though on finely speculate grounds and in a manner vague, and in many ways unsatisfactory and absurd. Kant, in 1755, independently and more fully developed similar ideas, and avowedly based his speculations on the Newtonian laws of motion and gravitation, but his mathematics was hardly equal to the subject, and some of his ideas are simply absurd from the mechanical point of view. Thus he makes his original nebular absolutely homogeneous and quiescent, and makes it set itself to whirling by a mere process of contraction. His speculations upon the inhabitants of the different planets are, of course, worthless. It is not certain whether Laplace (1796 to 1808) had or had not some previous knowledge of the speculations of Swedenborg and Kant. However, in his work we do not find any mathematical or mechanical absurdities, but the theory of heat was not then understood and the more modern science of thermo-dynamics requires some very essential modifications of his views. More recent workers along the line are Roche of Montpelier; Faye, who

has proposed a very essential but not widely accepted modification of the hypothesis; Proctor and Lockyer, who have emphasized the role of meteors in building up suns and planets, and Darwin, who has shown that a cloud of meteors on a large scale has a tendency to behave in its progress of aggregation and central condensation precisely like a finely gaseous mass. He has brought also the effect of tidal reactions between a central mass and planets circulating near it.

At the business meeting, Mr. Silas Wodell, and Dr. Theodore Neumann were unanimously elected members of the Institute.

Dr. Stevenson nominated Mr. H. P. Amen, and Mr. Elsworth nominated Miss A. M. Goodsell for membership.

MAY 6, 1890—NINTH ANNUAL MEETING.

President Henry V. Pelton, and a quorum of members present.

Miss A. M. Goodsell and Mr. H. P. Amen were respectively elected members of the Institute.

President Henry V. Pelton presented his report of the work of the Institute as follows:

Your president respectfully presents his report of the work of the Institute and of its sections, for the past year.

Five public meetings of the Institute were held, covering all the dates for which the by-laws provide, except the one in January, when it was thought best to omit the meeting.

Your president presented the address on November 12, as required.

December 3. William A. Mowry of Boston lectured upon "The Early Settlement of the North West Territory."

February 4. Rev. George D. Hulst of Brooklyn, N. Y., spoke upon "Insects and their Habits."

May 4. W. Le Conte Stevens of Brooklyn, N. Y., delivered an illustrated lecture on "Sensitive Flames and Sound Shadows."

April. 1. Prof. Charles A. Young of Princeton, New Jersey, lectured on "Nebulæ and the Nebular Hypothesis." This also being illustrated.

At none of these meetings was the attendance large and at some it was very meagre. It is the first time that the Institute has tried this method, of engaging a number of paid lecturers from other places, and certainly the interest manifested by the citizens of Poughkeepsie, was quite unworthy of the character of the lectures thus provided and it was a disappointment to those who had arranged the course. Business meetings followed each of these public meetings, except one. Six members were elected during the year.

Only one section has been active during the year.

The Scientific Section held eight public meetings at which papers and addresses were presented by members, as follows :

1889.

November 19. Chairman Edward Elsworth.
"Some Suggestions concerning Food Adulterations."

December 10. Gilbert Van Ingen.
"The Ferns of Poughkeepsie."

1890.

January 14. Dr. W. G. Stevenson.
"The Imagination."

January 28. Charles B. Warring, Ph. D.
"What keeps the Bi-cycle from Overturning."

February 18. Le Roy C. Cooley, Ph. D.
"Electricity—What is it?"

March 11. Gilbert Van Ingen reported two Land Snails found in Poughkeepsie, that were Foreign to America.

March 25. Gilbert Van Ingen.
"Land Snails of Poughkeepsie."

April 8. Charles B. Warring.
"Sir Wm. Thompson and his Gyrostatic balance."

Your president is not aware of any meeting of either the Library or the Art Sections being held during the year.

Other matter connected with the Institute during the past year will be included in the other reports to be presented this evening, and need not be referred to here.

HENRY V. PELTON,
President.

The treasurer Mr. Edward Elsworth, submitted his report of the receipts and expenditures during the year. The following is a Summary thereof:

Balance in Treasury May 7, 1889	$2,420 02
Total Receipts during the year	1,671 78
Total	4,091 80
Total expenditures during year	1,827 01
Balance in Treasury May 2, 1890	$2,264 79

The chairman of the board of trustees, Le Roy C. Cooley, submitted his annual report concerning the condition of the property.

The following officers were unanimously elected for 1890–1891:

TRUSTEES.

CHARLES N. ARNOLD,
LE ROY C. COOLEY,
WILLIAM B. DWIGHT,
EDWARD ELSWORTH,
IRVING ELTING,
CHARLES B. HERRICK,
HENRY V. PELTON,
WILLIAM I. REYNOLDS,
WILLIAM G. STEVENSON,
A. P. VAN GIESON,
CHARLES B. WARRING,
EVAN R. WILLIAMS.

President,	MR. HENRY V. PELTON.
Vice-President,	MR. CHARLES B. HERRICK.
Secretary,	MR. JAMES WINNE.
Treasurer,	MR. EDWARD ELSWORTH.

WILLIAM GEORGE STEVENSON, M.D.

Doctor Stevenson was born in 1843, near Sandusky, Ohio, whither his father, a prominent physician of Cambridge, Washington County, N. Y., had emigrated a short time previously. After a brief experience at the west, the father returned to Cambridge, where the younger Stevenson grew up and received an academic education. He studied medicine with his father, and became a student in "The College of Physicians and Surgeons of the city of New York," from which institution he graduated in 1865. After a course of practice in Bellevue Hospital, New York, he returned to Cambridge, and followed his profession there with brief interruptions, until 1872, when he removed to Poughkeepsie, where he continued to reside until the time of his death.

As a physician he has ever occupied a very high position, not only on account of his intelligence and skill, but also because of the fertility of his resources, and the rare courage combined with gentleness, with which he combatted disease.

He was a zealous worker in literary and scientific fields, and the literature of his profession has been greatly enriched by his contributions to various scientific and medical magazines.

In the best sense, he was a *citizen*, and found time during the exacting duties of an extended practice, to devote much attention to public affairs. He served as a member of the Poughkeepsie Board of Health for three years, as medical examiner of applicants for pensions, under President Cleveland, and for the five years preceding his death, as member of the Poughkeepsie Board of Education. In him the schools of his adopted city have had a most efficient friend, and improved courses of study, and great improvement in the sanitary arrangement of the school buildings of the city, have been the

direct result of his vigorous and intelligent efforts. He was well known in scientific circles, throughout this country, and in Europe, being a manager of the American Society of Natural Science, and a member of the Medico-Legal Society of New York.

As a member of the late Poughkeepsie Society of Natural Science, and of the Poughkeepsie Literary Club, he was chiefly instrumental in bringing to the attention of the Vassar Brothers, the project of uniting those two societies. The result was—"Vassar Brothers Institute" with its liberal endowment. He held the office of President of the Institute from 1885 to 1887, and was Curator of the Museum at the time of his death. The Museum and Library are chiefly the result of his individual work.

Dr. Stevenson's well stored mind, quick grasp of facts, and overflowing humor, free from any taint of acrimony, made him a brilliant conversationalist, and a delightful companion. He was also a man of firm convictions, and great capacity for hard work, which he did not relinquish until long after fatal disease had seized him, nor until death was near. He never touched a subject without leaving his impress upon it, and the impress was always for good.

At a special meeting of the Institute held Tuesday evening, October 14th, 1890, the following resolution was unanimously adopted:

Resolved, That we, the members of Vassar Brothers Institute hereby record upon the minutes of the Society, our sense of the grievous loss which the Institute has sustained by reason of the death of its former president, and late curator of the Museum, Dr. William G. Stevenson.

Whether in public or private life, his character and his devotion to duty, alike commend themselves to us, for the first was pure, the latter unselfish.

His contributions to the cause of science, his zealous service in behalf of public education, and his tireless and skillful efforts for the relief of human suffering, are a rich legacy to this community.

His death is to this Institute a loss for which we may look in vain for compensation. Its interests and welfare were ever in his mind, and almost his last utterances were in its behalf.

The Museum may be regarded as his monument, for it owes to him its organization. He brought order out of chaos, and its growth, arrangement, and classification are the result of his directing intelligence, and of many years of personal toil.

As he was ever an earnest seeker after truth, truth found in him a noble exponent, No one ever gave him his friendship without receiving from him the most complete surrender of himself. For a friend, no service was too onerous, no sacrifice too great.

As we take up the work which he has laid down, we shall more and more realize what he was to this Society. That we may emulate his perseverance and industry, his skill and integrity may rightfully be the measure of our ambition as members of the Institute.

At the Special meeting held October 14, 1890, a quorum being present, Mr. Edward Burgess was elected Secretary of the Institute, vice James Winne, resigned.

SCIENTIFIC PAPERS

OF

VASSAR BROTHERS INSTITUTE,

AND

TRANSACTIONS

OF ITS

SCIENTIFIC SECTION.

1887-1890.

VOL. V. PART II.

TRANSACTIONS

OF THE

SCIENTIFIC SECTION.

1887–1890.

DECEMBER 6, 1887—FIFTY-SEVENTH REGULAR MEETING.

Mr. C. N. Arnold, presiding ; eight members and several guests present.

The chairman, Mr. C. N. Arnold, gave an informal address on "Woods and Gums," exhibiting a collection of gums ; also of mounted microscopic specimens of woods.

Prof. Dwight reported the reception of twenty-one Haytian coins, and a complete list of the same, the gift of Dr. L. H. Junghanns.

A specimen of adipocere from the "Old English" Burying ground was donated by W. G. Stevenson, M.D.

DECEMBER 20, 1887—FIFTY-EIGHTH REGULAR MEETING.

W. G. Stevenson, M.D., chairman *pro tem.*, presiding; ten members and fourteen guests present.

The following paper was read :

MIRACLE, LAW, EVOLUTION.

BY CHARLES B. WARRING, Ph.D.

I. *Miracles and Law.*

I do not propose to discuss the reality of the miracles recorded in the Bible. Those who deny their possibility, as well as many who believe in them, look upon miracles as generically different from those occurrences which are said to take place in obedience to the laws of nature. There certainly is a difference. What is it ? Evidently

it is not in the degree of power required; for raising the dead is a less thing than the birth and growth of an individual. It is less to set in motion a watch that has stopped than to make one. Causing the widow's oil and meal to increase was a small matter in comparison with providing daily food for the millions that cover the earth.

Nor is it that miracles are intrinsically more wonderful than those things which we regard as the effect of law. Nothing is more common, or less excites our surprise, than the working of the law of gravitation; yet, what is more wonderful than a force which, according to Laplace, travels more than fifty million times faster than light, itself moving with the inconceivable velocity of nearly two hundred thousand miles in a second?

Were, to-night, in some far distant constellation, another sun called into existence, ages would pass before its light could reach our earth, but only a few seconds would elapse before the earth would feel its presence.

Nor is the velocity of gravitation the only or the greatest cause for wonder. Another property of this force far surpasses that. I mean its ability to adjust itself with omniscient accuracy to every change in mass, or position, of bodies however widely separated.

The adjustment of position and movement of every member of our solar system, and, I may add, of the universe, to the mass, distance, and position of that new sun would at once begin and, in due time, complete itself with an exactness which no human measurements can hope to equal. And since every atom is attracted by every other atom in the universe, it is a sober fact that the fall of a sparrow is registered in every star fifty millions of times sooner than light can speed its way across the abyss that separates our earth from them. Can any miracle be more wonderful than that?

We may extend the comparison as far as we please, and we shall find in all cases that miracles differ from

what we regard as the effect of law, neither in the amount of power required nor in their intrinsic wonderfulness.

We must, therefore, seek for some other characteristic by which they may be distinguished. So far as I can see, this lies in the continuity of the one, and the absence of continuity of the other. In a world where no vegetation existed, the production of an oak would be a miracle. To us it is merely the outworking of law, because it goes on continuously. The first plants and the first animals came into existence by a miracle. They continue to come into being, and now it is law. In every law the first of the series was a miracle, and, had we been present, would have excited our profound wonder. But often repeated, it ceases to excite surprise, yet the thing itself is unchanged.

The dead rising, the deaf hearing, the blind seeing, when commanded by Christ or in His name, were miraculous occurrences. But if this had continued, if every time a dead man was told in that name to rise, or a deaf man to hear, or a blind man to see, he had obeyed, no more surprise or wonder would be excited than now that men wake from sleep. It would be simply the way in which nature works.

So far, then, as I can see, the peculiarity of miracles lies in their uniqueness. Each stands by itself. A miracle may be represented by a point; law by a line; successive points make a line. Miracles indefinitely repeated crystalize into law. In briefest phase, law is continuous miracle. Those, therefore, err who rank miracles as higher or more divine than law; for as two are more than one, and four more than three, and a series greater than any one of its terms, so law is greater and intrinsically more wonderful than miracle. Neither admits of explanation other than the will of that First Cause which lies back of all.

II. *Miracles and Evolution.*

What, it may be asked, have these to do with each other? Are they not catch-words that represent the opposite poles of modern thought? I ask your patience and attention, and, when I have done, I hope it may be clear that they do have something to do with each other, and that, if they represent opposite poles of thought, they help to weld fragmentary truths into one consistent whole.

Christ is represented in the Bible not only as a worker of miracles, but as the informing spirit that produced all things which exist, or ever did exist; the Creator.

Those who believe this, and that includes all who regard the New Testament as inspired, will, I think, agree with me when I say we may reasonably expect to obtain from the study of His methods, when performing His miracles, some light as to His course when acting in His capacity of creator. Just as when we find certain peculiarities of style or diction in one of an author's books, we expect to find them to some extent, at least, in all his works; so, if we find some peculiarity, some way of doing things that runs through all His miracles, it would be reasonable to look for it in His mode of doing His creative work. And I think we are safe in saying that of two theories as to how certain things were done, *e. g.*, how present animals came into the world, that which harmonizes best with His methods when working His miracles would be the most likely to be true.

A brief study of the accounts which we have, will suffice to show that, in Christ's miracles, natural means, laws, and powers go just as far as is possible for them, and then the supernatural comes in and does what they cannot do. When, for example, at the marriage in Cana, Christ would supply the lack of wine, there was the water, the jars, the servants, all in the usual way, He bade

the servants fill the jars with water. They did that, and it was as far as natural means could go ; then came in His power and added the components needed to make the water wine. This done, the supernatural ceased, the natural again came into operation, for it was the servants that drew out the wine and bore it to the governor of the feast.

Would he feed the hungry thousands? He commanded them to sit down, took the bread and fishes, broke them, and gave to the disciples to distribute. Thus far all was in accordance with law, and it was as far as law could go. At that moment, the divine power came in and did the one thing impossible for nature : it caused the bread and fishes to multiply.

Even here he kept as close to the natural method as was possible. Would we have an increase in our stock of food, we take wheat, and it produces wheat, or barley, and it produces barley. In all cases by natural law, we take a portion of that of which we would have more, and it multiplies. So here. He would have more bread, he took that, and it produced its own kind. He would have more of the fishes, and the animal fabric grew under his hands, and from what he had, came more of the same kind. It was like producing like, in a strange, abnormal way, it is true, but no more inexplicable, in the last analysis, than is now what we call the natural process by which we get our food.

After the increase of the loaves and fishes, there was no further need of miracle, all else proceeded in the usual way. The multitude ate and were refreshed in the ordinary manner, and the disciples gathered up the fragments.

Would He supply tribute money for His disciples? He told them what they could have learned from no power in nature, viz., where to throw the line to catch the fish which had seized the glittering coin, as it sank in the

water of the lake. Up to a certain point, all was natural, the hungry fish voraciously seizing the sinking coin, the line, the fisherman. The one thing needed to complete the transaction, the omniscience which notes the fall of a sparrow, was supplied by Christ. The disciples did the rest. They took the money and paid the tribute.

When He raised the dead child, He brought back life and health, but He commanded those present to give her food. She got life from Him, but strength she was to get in the ordinary way.

When He restored Lazarus to his sisters, He bade the Jews standing around roll the stone away from the tomb. Then they reached their limit. At this instant of absolute helplessness, when nature and man could do nothing, He interposed and gave life to the dead body, but it was the living Lazarus himself that walked out of the sepulchre, and it was those standing by who loosed him from the grave clothes. Christ's exercise of miraculous power, here also, was confined to the one thing law could not do, the restoration of life.

All the other miracles, so far as we can judge from the very brief records, are marked by the same characteristic.

Hence, I think, we may conclude that the divine method in miracle-working was to do only that which Nature, with her laws and powers, could not do, and then to let her do the rest. What was already in existence was invariably used, as far as it could be applied, and to this was added only that which was necessary to complete the transaction.

Hence it seems reasonable to infer that, in His work as Creator, He used whatever was nearest to His purpose, and exerted power above Nature only to do what she could not. As to the rest, He left it to the outworking of natural causes.

Now for the application.

The world of to-day contains many thousand species

of plants and animals. There is indisputable evidence that the present is only the last of a long series of "populations," each differing from its immediate predecessor. Each antecedent population was of a lower grade than its successor, until at last we reach the dawn of life where only the lowest orders are found. Or, conversely, starting at the beginning of life, there were for millions of years radiates, articulates, and mollusks, but no vertebrates; then for other millions, there were water vertebrates, but none on the land; then for thousands of centuries, land vertebrates but no mammals, and for another long period, mammals, but none of existing kinds, and lastly those now living.

One example will suffice, although it reaches back but a little way, yet far enough for my present purpose.

Many thousand years ago, there lived an animal which geologists have named Orohippus, or the Mountain Horse. It was about the size of a very small Shetland pony, which in many respects it resembled. Still it was not a horse, for it had four little hoofs on each fore foot, and three on each of its hind ones. The genus lived many thousand years, each generation like its predecessor; but at last "from some cause unknown to science," a new animal, in fact a new genus, appeared, different in some respects from the Orohippus, and approximating somewhat more to the present horse, yet not a horse, for on each of its feet were three hoofs. The Mesohippus, for so geologists have named it, also kept on for many generations, producing at every birth only its own likeness. After a uniform course, for we know not how many thousands of years, there appeared another crea ture, the Miohippus, very much like its predecessor, but approaching more nearly to the horse. The middle hoof was larger, indicating a promise of an animal in which the two side hoofs should disappear. The Miohippus lived from generation to generation its uneventful life,

one monotonous series of like producing like, till at last another animal made its appearance still in the same line of progress ; the side hoofs remained, but of diminished size, while the teeth became more like those of the horse.

This genus (Protohippus) ran its course, and then another (Pliohippus) came into existence with greater resemblance to the horse, for it had single hoofs, and teeth still more equine.

Next and last came the horse, the living servant of man.

It is not possible as yet to trace the pedigree of any other animal as satisfactorily as this ; there is, however, sufficient evidence to induce the belief that there has been a similar process in all species.

The question is how to explain these facts. Scarce any one doubts that the first life came direct from the Creator. It is in regard to the subsequent populations that biologists differ. The fact of there having been such is beyond dispute. It is as to the manner of the successive genera coming into existence that there is question. Only two suppositions are conceivable. Either each species was made *de novo* by the Almighty, or it was born of some preceding creature of a different species. The former is the older theory, and claims to be in exclusive harmony with sacred writ. It teaches that God made, *e. g.*, the Orohippus, from earth, air, and water, and gave it life ; that later, from more of the same raw materials, he made the Mesohippus ; and yet later, from more earth, air and water, he made the Miohippus ; that, after another long interval, once more from earth, air, and water, God made the Protohippus, and so on down to the present horse. There was a succession of creations, but no genetic relation between them.

The other theory also holds to the belief in a Creator. It, however, teaches that only the first kinds of plants and animals were made direct from inorganic material.

It thus accounts for the first links in the chain of life, but claims that, from these, others of new and different kinds were produced at some subsequent time, and from them others, and so on down through many stages to the present. It holds that the law of like producing like was then as now the law, till time and environments were ready, perhaps after thousands of generations, and that then "some cause unknown to science," an agnostic euphemism for a more or less direct act of the Creator, so changed the factors in what may be called the personal formula of the embryos,[1] that they grew up into animals of species till then unknown. Thus, for example, "some cause unknown to science" so changed the embryo in an Orohippus, that it was born a Mesohippus; and, after many thousand generations of the new species, like begetting like for all that time, the "cause unknown to science" so changed the embryo a second time that from the Mesohippus was born a Miohippus, as if now from a panther a lion should be born, and thus the process went on.

The first of these theories, a creation *de novo* for each new species, is as unlike the course of Christ in His miracles as possible. He employed what was already in existence and nearest to his purpose, and put forth the least possible amount of divine, or extra-natural, power that would suffice to adapt the same to his design. The production of new species by changes in preceding forms nearest related, appears to be in perfect harmony with His method while on earth as Son of Man.

Such derivation of new species from older species is the essence of evolution, and this, whether the evolution was by imperceptible degrees as taught by Mr. Darwin, or at once, per *saltum*, a bound, as it were, at one birth, or at most, in a few successive births, from the old to the

1 "The transition from type to type was done during fœtal life." Cope, *Origin of the Fittest*, page 276.

new. Such abrupt changes seem most in harmony with the teachings of the miracles. In these the thing to be done was done not imperceptibly, but at once.

So far as I can read the record of geology, its evidence also is in favor of abrupt changes. The links in the pedigree of the horse are well defined; there is not an imperceptible, long-continued transition from genus to genus. There was the Orohippus, and, after a time, suddenly the Mesohippus appears; again, there was generation after generation of the Mesohippus, and then, all at once, is found the Pliohippus, and so on.

In spite of an original and very strong bias the other way, biologists now admit the occurrence of sudden starts upward, jumps in the progress of development. Prof. Huxley, in his "Lay Sermons," page 297, says: "We believe that nature does make jumps now and then." May we not ask: Is there conclusive proof she ever does otherwise? He adds: "Mr. Darwin embarrassed himself with the aphorism which turns up so often in his pages, 'Natura non facit saltum.'"

Prof. Cope, "Origin of the Fittest," page 123, says: "The results of such successional (embryonic) metamorphoses are expressed in geological history by more or less abrupt transitions, rather than by uniformly gradual successions." It is difficult to avoid the belief that, but for theoretical reasons, biologists, in reference to new species, would almost adopt the motto, Natura semper facit saltum. Be this as it may, evolution gives us no aid in accounting for the changes. The survival of the fittest, however important in determining what varieties shall survive, gives no assistance in determining how and why the variations occurred. As Prof. Huxley well says: "What the hypothesis of evolution wants is a good theory of variation." At present it can be attributed to nothing more definite than "some cause unknown to

science." Miracles are equally well explained in the same way.

The believer in the Bible will ask: "But does not this conflict with the story of creation in Genesis? If Genesis be true, is it possible that present species of animals are descended from other species, and back through many steps to the first stages of life upon our globe?" But wherein is the contradiction? Genesis says only that God made, or created, the various creatures named. As to how He did it, there is absolute silence, hence contradiction is impossible.

The chief interest most persons have in evolution pertains to man's origin. As to his higher part, the soul, few will be found to deny that it came direct from God.

The doubt is as to his body. Did God form it directly from the ground and atmosphere, moulding the mixture to his purpose, and then give it life? Or did He take, in embryo, or after birth, some animal nearest to his design, and enlarge its form, shorten the length of its arms, change its hand-like feet till fitted for man's upright position, and enlarge the capacity of the skull to fit it for the large brain which was to be the facile instrument of the soul, the go-between of the soul and the body? Whichever really was the mode of man's creation, there can be, I think, no doubt that the latter is most in harmony with Christ's methods when exercising His power in the miracles. And, as for the question of dignity, surely matter which under the divine hand had been prepared and refined in all the infinitely delicate machinery of a living body, though that of a brute, was, to say the least, as worthy of man as that which had never since its creation received the divine touch, but had lain, raw and crude, beneath the feet of man's predecessors.

Then there is the creation of Eve. God undoubtedly might have made her as he did Adam. But in accordance with the law that runs through the miracles, it

would seem probable that God took material nearest fitted for his purpose, and in harmony with a method found all through nature, propagation by fission, caused from a part of the man a woman to grow. It seems to me that this was closer to nature's methods still in operation—like producing like—than such changes in another simian, as occurred when one changed to a human being, and became in the sense which we all understand, but shall never comprehend, a living soul.

It will seem strange to many that the study of the miracles of the Bible has led to the support of evolution, so strange, that I fear it may hinder their giving due weight to the argument. But, although some advocates of evolution may appear to desire to use this theory to shut the Creator out of his own world, it seems to me that species produced by modification of previous species show His hand as unmistakably, nay, I would say, show it more clearly than if moulded direct from earth, air, and water.

The paper was discussed by Dr. Stevenson, Professor L. C. Cooley, Rev. Dr. Brown, and Mr. Bartlett.

JANUARY 10, 1888—FIFTY-NINTH REGULAR MEETING.

Charles N. Arnold, chairman, presiding; ten members and several guests present.

Prof. Wm. B. Dwight, exhibited a working model of his new section cutter, and spoke as follows concerning:

SOME PRACTICAL SUGGESTIONS AS TO THE PREPARATION OF THIN SECTIONS OF ROCKS AND MINERALS.

BY W. B. DWIGHT.

The object of this paper is to call attention to two tendencies to error in the manipulation of the preparation of thin microscopic sections of paleontological, and

probably also to some extent, of lithological specimens, as generally practised. Doubtless there are scientists to whose work in this line these remarks will not apply ; but there is reason to think that they are exceptions.

The first erroneous tendency is the supposition that the usefulness of a section will be very much in proportion to its thinness ; that is, "the thinner, the better." To follow any such imperative rule is calamitous, for the following reason :—the object of slicing specimens is to obtain thereby as complete a knowledge as possible of every detail of the internal structure. In most objects of paleontological interest, and perhaps in many of lithological study, there will be details of coarser structure intermixed with details of exceedingly delicate structure. There may also be details of delicacy intermediate between these two. It is only the amount of contrast between the light and shades of the details in the finished section which will develop the structure. If a certain tissue is exceedingly delicate, it may be possible, at a certain rather thick stage of the thinning of the section, for this tissue or structure to be thin enough to admit light and yet thick enough to show itself by contrast with the surrounding "filling" of the specimen ; whereas a little more thinning of the whole may, and probably will, so thin this particular tissue that there is not enough of its thickness left to present any contrast with its surroundings, and the knowledge of its presence will be entirely lost. Yet at this thicker stage of the section, the coarser portions of the structure may not be sufficiently thinned for study.

The usual practice has too often been to watch only the more prominent outlines of structure, to thin the section until these are as transparent or translucent as possible, which generally means to make the section as thin as possible before stopping. But by this time all

the delicate features are obliterated, and even the delicate inner structure of the coarser outlines is gone.

Let me mention a practical illustration. I have been studying an obscure microscopic coral by slicing it. It had ben considerably studied, both in Canada and in Europe, by eminent scientists who had failed to find any *tabulæ* and who had so reported. In my earlier sections, where I fell into this common error of extreme thinness, I found none; subsequently, by leaving my sections at a thicker stage, I found the tabulæ standing out in absolute clearness. Some of the scientists who had been unable previously to discover them ; after seeing my own specimens, were able to barely distinguish them also in their thin specimens, by very careful scrutiny.

The rule of practice is quite evident. No one stage of thinness will suffice for both the coarser, and the more delicate structures. Watch the slice carefully as it becomes more and more translucent, studying especially the *more delicate* structures first. To develop these, stop at exactly the point where more thinning seems not to bring them out better, but to lessen their clearness of contrast: Lay this slice aside as one specimen ; prepare other slices with a greater degree of thinness, to develop better the coarser structures. The conditions of each kind of specimen can alone determine how many slices, of as many different grades of thinness, may be required to develop completely the structure of any one object.

Another remark is here important. To some extent this precaution may often be sufficiently observed even where only a *single slice* is made ; as for instance when the material is scarce, or where the object really cannot be duplicated. It may be done in this way: stop the general grinding and thinning of the slice on the plate glass, exactly the point where the more delicate tissues have their maximum of clearness. Then, by using a proper tool, thin out by delicate manipulation only the

more opaque details, until their structure is also sufficiently translucent, avoiding any further abrasion of the delicate structure. This may be done by using a small stick, having a very small squared end, but with its edges a little rounded. Have a little emery mud in a saucer near your hand; dip the end of the stick in it, and gently rub it on the portion of the structure to be further abraded, holding the specimen in the left hand, against the light, so that you can see exactly where to work. For very narrow structures, the end of a common square match-stick is of the right size; but often a larger stick is better. Still better, where it is not too large, the flat head of a small copper nail is excellent, as it works rapidly. But frequent rinsing of the slice will be necessary to examine the progress of the work, and much care to avoid abrading a hole in the thinner contiguous portions.

The second infelicity in the general methods of rock-slicing is the contentment with the preparation of the usual very small sections, rarely more than seven-eighths of an inch square, often considerably smaller. The New York State Survey is doing excellent work in the preparation of very large sections, which is greatly to be commended. It is possible that the United States Geological Survey may also prepare large sections; but the general apathy of scientists with respect to the importance of such sections is strange.

It seems to be the common, but entirely erroneous, assumption that a small slice, no bigger than a thumb nail, may be considered a fair sample of a specimen which is, perhaps, as big as a fist. To a very considerable extent this is very probably true in the making of lithological slices; though even in this special line I believe it to be a wrong assumption with reference to certain points of research. This question will have further consideration a little later.

But in slices of paleontological specimens, there can be no doubt not only that very large sections—that is, of from two to four square inches—are much to be preferred where attainable, but that with reference to many classes of organizations, *they are the only slices* upon which a true knowledge of the organism can be predicated.

Two chief reasons may be presented in justification of this assertion.

First—It is impossible to tell beforehand what portion of the paleontological specimen may microscopically present the structure of the organism in its most complete form, or in the best state of preparation. Chipping off a small splinter and slicing it is simply a lottery, not up to the dignity of true scientific research. It may hit a very perfect portion of the structure, or it may hit a spot, which, however favorable it appeared macroscopically, is inferior, microscopically, to other parts of the specimen.

But if a large slice, of two, three or four square inches, is made, a large field is at once presented, from which a *selection* can be made of the best-preserved portions, if it is not desirable to mount the entire slice.

In the second place, the different variations in the type of the structure may be entirely lost in the smaller fragment while the larger section may present them in combination, and thus prove their unity and the identity of the species.

I may illustrate this best by an actual experience of my own. In endeavoring to study some fossiliferous Trenton specimens, containing the same microscopic coral above mentioned, there appeared, to myself and other paleontologists, to be at least three separate species present; one, a sponge, and two corals of a different genera. There was nothing to disturb the view so long as slices of the ordinary small size were made. But as soon as large sections of a single coral-colony were made, it be-

came evident that these three forms were all protean and remarkable variations of the same species of coral.

With regard to petrographical specimens, while, as above stated, it may be conceded that in many cases a small chip would be a sufficiently fair specimen of the mass, this is not true in certain special lines of research.

Consider, for instance, a case where the metamorphic alteration, or alteration by decomposition, of a certain kind of rock, is the subject of study. A small chip taken off accidentally, showing some most convenient edge, may fail entirely to exhibit some of the more instructive changes of the specimen. But if one or more large sections are made, and these large fields are examined, the most interesting and instructive points can be picked out, and there is far less chance that some of the most important exhibits may escape our vision.

It may be supposed that the cutting of the large slices is generally impracticable. Such is not the case. With proper machinery (one form of which will be described in another paper), it is quite practicable and easy. The cutting of a large specimen of a limestone rock, or any soft rock, takes very little more time than the cutting of a small one, and the grinding down of either takes practically about the same amount of exertion.

I urge very strongly the propriety of the general preparation of much larger sections of paleontological specimens, and of certain groups of petrographical specimens than the usual sections now made.

THE EXHIBITION AND DESCRIPTION OF NEW DEVICES IN MACHINERY FOR THE MAKING OF THIN SECTIONS OF MINERALS AND PALEONTOLOGICAL SPECIMENS.

BY WILLIAM B. DWIGHT.

After making some preliminary statements as to the entire lack of any machine capable of cutting thin slices

of rock material, of large size, as well as in small fragments, with precision, economy of material, and rapidity, the lecturer exhibited and described a machine, devised in large manner by himself, which is adapted to accomplish these objects.

Several years ago, a slicing machine was devised by Professor James Hall, the eminent State Geologist, with the assistance of his son. This was a long distance ahead of the ineffective and crude "lapidary's lathe" used by most scientists who make rock sections.

This machine has been used in the State Geological Survey, and very large and fine sections of fossils have been made by it with considerable facility. It was not, however, adjustable, so as to cut desired planes with sufficient precision and delicacy, and the adjustments of which it was capable consumed much more time and work than was either necessary or desirable. There was too much waste of both time and of valuable material in its use. Nevertheless, it was a great step in advance; the main principle of its construction is very probably the very best that can be devised; only its capabilities were not worked up sufficiently into the details of exact work.

The lecturer, finding some machine of this kind absolutely necessary for his own paleontological work, but finding also that the Hall machine would not accomplish what was desired with sufficient exactness, undertook, as a matter of necessity rather than of choice, to start with this good basis of a practical machine already devised and working it up in detail, to produce something which might more or less deserve the name of an instrument of precision. He, however, acknowledged, most heartily and gratefully, his indebtedness to Professor James Hall and his son for the fundamental ideas and movements which have made it possible to elaborate an instrument which can do precise and economical work.

For the present machine for rock-slicing, the following powers are claimed, possessed, it is believed, by no other machine of the kind:

1. Great facility in grasping or clamping with firmness specimens of every variety of size and shape with very little expenditure of time.

2. A high degree of precision, and yet of rapidity of adjustment of the desired plane of section to the plane of the cutting disc.

3. An absolute control over the level of the large and thin cutting disc, which should be eight inches or more in diameter and as thin as ordinary tin plate. This control compels it, however warped, to cut a perfectly true plane, in large specimens of several square inches.

4. Capacity, by reason of the above arrangements, to cut sections of several square inches in such a degree of thinness that very little subsequent grinding is required.

It would be impossible, without a large number of illustrations and a lengthy description, to give any adequate idea of the details of the mechanism. The following very general description will suffice to indicate the character of the adjustments and the methods of control:

Upon the bed of a strong lathe, a large cast iron table is firmly bolted; the surface of this table is machine-planed to a true level. Upon the front left-hand corner of this table (*i. e.*, left hand of the operator), is bolted the standard-frame carrying the standard vertical bar turning upon centers. A stout cylindical horizontal bar, carrying the jaws, has a vertical motion upon this standard bar, adjustable by a long vertical screw, while it also has a horizontal motion by the turning of the standard upon its centers. This general arrangement of the standard and horizontal bars formed the basis of the original device of Professor Hall.

The jaws have a regulated movement along the horizontal bar, and at the same time a rotary movement by

cog wheels or worm-gearing around their own horizontal axis; each jaw is also separately adjustable, to and fro, by a powerful screw. There are a number of additional adjustments and accessories, by the use of which the desired plane of section of any specimen, clamped in the jaws, can be quickly brought into exact parallellism with the surface of the planed iron table, which is the base level to which all the movements are referred. A Starrett's surface-gauge is used in these adjustments. Various forms of special clamps are provided, adapted for certain forms which are specially difficult to grasp.

The thin, and always more or less warped, cutting disc, charged along its edge with diamond powder, is held in its true level, along the cutting portion, by a peculiar contrivance of swinging arms carrying adjustable vertical rollers, ten or more in number, half of these above and half of them beneath the disc. This device, called the "cutting-disc guide," proves to be very effective.

The method by which the disc is more or less permanently charged with diamond powder is a recent German invention, apparently little known, if at all, to our lapidaries, but in use by our lithologists, and much prized by them.

All the parts of the machine are made with great exactness, and the very best workmanship; otherwise it would be of little value.

Although the mechanism appears somewhat complicated, and the various accessories contribute to this appearance, yet it is a fact that there is very little liability to get out of order, and the actual working of the machine is exceedingly simple.

Pupils who have no special acquaintance with machinery yet learn very quickly to cut large sections with it with perfect readiness and success.

This machine is not only destined to become useful to scientists, but it also has probably useful industrial ap-

plications. The method by which these circular saws can be made to cut in true planes, is applicable as well to the large saws which would be needed for the sawing of marble slabs as to the smaller ones used in cutting lithological sections.

JANUARY 24, 1888—SIXTIETH REGULAR MEETING.

Charles N. Arnold, presiding.

A letter from Dr. A. T. Woodward, of Brandon, Vt., concerning the "Frozen Well," was read and discussed informally by the members.

Dr. C. B. Warring also presented a brief paper upon "Moonlight and Electric Light," as follows:

MOONLIGHT AND ELECTRIC LIGHT.

A few days ago, on Tuesday evening, November 29th, the air was very clear and a little cold, the thermometer standing at 30°. The moon was sensibly full, and at a rough guess 30° above the horizon. It occurred to me that it was a good time to make an approximate comparison of the electric light with that of the moon. The former was the arc light, and was rated at 1,200 candle-power. The burning carbons were protected by a large globe of clear (*i. e.*, unground) glass. The lamp is about 22 feet above the ground.

The first comparison measurement was made at 7 o'clock, by the method of shadows. I carefully paced a distance from the lamp till the shadows of a lead pencil made by the moon and the lamp falling side by side on white paper were of equal intensity. To get this point more accurately, I moved toward the lamp till the lamp shadow was decidedly the darker, and then did the same for the shadow made by the moon, and after each moving went back to point of equality. The results came out with surprising agreement. The result of all was that the point of equality was 60 paces from the foot of the

pole of the lamp, or 60½ paces from the lamp itself. This gives the light of the full moon at that hour equal to 49,280,000,000,000, or, in round numbers, 50,000,000,000,000 times the electric light.

Two hours later I repeated the measurements, and, as might have been expected from the greater altitude of the moon, the result was greater, the shadow being equal at 55 paces. This gives 58,522,500,000,000, which means that this number of electric lights placed 240,000 miles away would give as much light as the moon.

I have called a pace 36 inches, which is quite close to the fact. If we call it 34 inches, we shall have to diminish the result by $\frac{1}{9}$, leaving in the first case the moonlight greater in the ratio of 44,000,000,000,000, and in the second case 52,670,000,000,000. The latter is nearer the true ratio when the full moon is in the meridian. To put it in another form, one electric light placed on every 225 square inches of the moon's surface, considering it a plane 2,000 miles in diameter, would light up the earth as well as does the full moon.

The area of a vertical section of the luminous arc is very small—one-half inch square I think a very liberal estimate. If this is correct, then the luminosity of the moon, inch for inch, is 900 to 1,000 times less than that of the electric arc.

To give one arc light of 1,200 candle-power requires three-fourths of a horse-power. To illuminate the earth from a point as distant as the moon, and to do it as well as is now done, would require 39,000,000,000,000 horse-power, or 30,000 horse-power for every man, woman and child on the globe.

FEBRUARY 4, 1888—SIXTY-FIRST REGULAR MEETING.

Le Roy C. Cooley, Ph.D., chairman *pro tem.*, presiding.

Prof. William B. Dwight gave a report of some recent discoveries by himself, as follows:

DISCOVERY OF A LOCALITY OF TRENTON LIMESTONE, RICH IN OSTRACOID ENTOMOSTRACA AND OTHER FOSSILS, AT PLEASANT VALLEY, N. Y.

While making field explorations at Pleasant Valley, six miles northeast of Poughkeepsie, during the autumn of 1887, I found a locality of Trenton limestone, containing a group of fossils, which appears to be unique in the stratigraphy of this county.

The locality is the deep cut in the limestone on the New York and Massachusetts Railroad (lately the Poughkeepsie, Hartford and Boston Railroad), situated close to the Pleasant Valley station, to the north of it.

The ledge consists of alternations of thick-bedded limestones (in some places of a smooth texture, in others very granular, tough, and gritty), with shaly layers of the same material. These calcareous shales are in places smooth and even slates, while in others they are exceedingly friable, and to a large extent broken up into small lenticular fragments. The smooth and fine-grained slates have not so far yielded any fossils; but the latter abound in some of the irregularly fractured shales, and in both the smooth and the gritty granular limestones.

At this locality, the strata have a strike about north 40° east, with a steep dip to the easterly.

That the limestone belongs to the Trenton epoch is made evident by the presence of the characteristic Trenton fossils of the vicinity, notably, Solenopora compacta and Orthis pectinella, in abundance. This identification is important, since a number of the other fossils present are heretofore credited to other epochs than the Trenton.

One of the most conspicuous paleontological features of the locality is the exhibition, at one or two points, of large surface covered with well-preserved specimens of

the organism described originally in the New State Reports as a fucoid, under the name "Phytopsis cellulosum." My own examination of these Pleasant Valley specimens makes it quite certain that it cannot be a fucoid, but that it is a coral of the genus Tetradium ; its correct name is, therefore, Tetradium cellulosum. It is assigned in the Reports to the Birdseye Limestone, and has generally been considered characteristic of that period ; but here it is certainly a Trenton fossil. Mr. C. E. Beecher informs me that he has found it also in the Hudson River Group in Kentucky. But the feature of chief interest is the occurrence of Ostracoid Entomostraca in great abundance, and in quite a number of different species, described and undescribed. Beyrichia striato-marginata, heretofore found only at Oxford, Ohio, is one of the most interesting of these. It is minute in size, being scarcely visible without a pocket magnifier. It is distinguished by its D-shape and its broad, finely-striated border. In Ohio, it is found in what is called the Cincinnati Group—the equivalent of the Trenton, Utica Slate, and Hudson River commingled.

Two other exceedingly minute species are found here, which are probably Isochilina. Leperditia fabulites, a well-known form found abundantly in Canada, and in the States along the Ohio River, occurs here.

The remaining ostracoids will require much study for complete identification, but the following Canadian species appear to be present : Leperditia Anticostiana, Canadensis, Josephiana, Louckiana and Ottawa ; also Isochilina gracilis. Some of these forms are credited in Canada to the Black River Limestone, some to the Chazy, some to the Utica Slate, Trenton and Hudson River Group. Here they are all in Trenton. Of the ostracoids which are probably new species, there is a fine tuberculated Beyrichia, and there are quite a number of

interesting species of Primitia, Beyrichia, Isochilina and Leperditia.

There are also a large and fine Plumulites, a new species; a new species of Metoptoma; a large and peculiar Pterotheca, a Murchisonia, perhaps new, and several interesting new forms of small trilobites. Among these are two species of Encrinurus, related to Encrinurus Mirus.

I have collected a large amount of this material for future study, as this very remarkable ledge is being gradually cut away and carried off in the improvements on the railway. Before long, the most fossiliferous portions will be probably entirely removed and covered up in the railway embankments.

MARCH 27, 1888—SIXTY-SECOND REGULAR MEETING.

Charles N. Arnold, chairman, presiding.

The "blizzard" of March 12-15 summarily postponed the address which was to have been given March 13.

Ten members and about fifty guests being present, Charles L. Bristol presented "Some Notes on Amateur Photography."

The paper was discussed by Professor Cooley, Mr. Elsworth and others, and an interesting exhibition of flash-light photography was given, followed by the development of the negative in the presence of the audience.

APRIL 10, 1888—SIXTY-THIRD REGULAR MEETING.

Charles N. Arnold, chairman, presiding; six members and several guests present.

The following paper was read by Professor Le Roy C. Cooley, Ph. D.:

LIMIT OF VISIBILITY FOR MINUTE MASSES.

BY LE ROY C. COOLEY, Ph.D.

By solution, a solid is broken into very minute particles which are uniformly distributed through the sol-

vent, separated by distances which depend on the number of particles in a given volume, and which become less as the quantity of the solid increases. There is a limit to the sensibility of the eye in the perception of minute magnitudes, and when a small mass of coloring matter is dissolved in a large volume of liquid its particles may be so small and the distances between them so large that the eye can receive no impression from them : the solution is then colorless. In a word, if the particles of a coloring matter in solution are too small to affect the eye separately, and the distances between them are not below the "limit of visibility," or the smallest distance which the eye can perceive, then the presence of the substance cannot be detected by vision.

By increasing the quantity of the coloring matter in a given volume of solution, its minute particles will be in closer proximity to one another. When the distances which separate the particles are reduced to the point at which they pass the "limit of visibility," then, if the masses of the particles are sufficiently large, the eye will perceive a continuous and uniform color. We may suppose the distances between these minute pieces of coloring matter in solution to remain uniform and constant in value, and, in this case, the intensity of the color perceived by the eye which receives transmitted light, must depend upon the masses of the particles. If these masses are sufficiently small, the luminous energy absorbed by their molecules will be too small in quantity to affect the eye, and the solution will be colorless ; but if, on the other hand, they are sufficiently large, the loss of luminous energy by absorption will be readily detected, and the corresponding color will be distinctly seen. It is evident that the masses may have a certain definite value, lying somewhere between the two values just described, which will enable them to produce a color of such intensity that the eye is just able to detect it.

This value will vary with the substance, inasmuch as molecular absorption by different kinds of matter is not the same. Moreover, it will vary with the sensitivity of the eye. But if in any case this value can be ascertained, it will, for that case at least, be the limit of visibility for mass.

The limit of visibility for linear magnitudes of various kinds has been estimated. For example, according to Tait, a difference in position of two lines on a vernier equal to $\frac{1}{0000}$ of an inch can be detected. Now we cannot declare that the particles of a coloring matter in solution, even when the quantity taxes the sensibility of the eye, are actually separated by this distance. But we are entitled to say that if the quantity dissolved in a given volume of solution be broken into particles whose masses have a certain small and uniform value, and be separated by distances uniformly equal to this limit of visibility, then the eye receiving light transmitted by the solution will perceive a color, the intensity of which it is just able to detect. Moreover, if the particles lie in closer proximity, as, in fact, they probably do, they must be more numerous, and their masses must be less in exactly the same proportion, so that the total mass within the distance of $\frac{1}{20000}$ of an inch—the limit of visibility—is the same. In every colored solution, the divisibility of the coloring matter must extend, at least, to particles whose mass is equal to the sum of all the smaller particles which, possibly, lie within the boundaries of the space whose size does not exceed the smallest which the eye is able to perceive. What, in any substance, is the actual value of one of these small masses?

In what follows, I attempt to answer this question by showing

1. How we may find the actual weight of the substance which, dissolved in a unit volume of solution, can be detected by the eye.

2. How we may ascertain the least number of the particles into which that weight must be divided in order to affect the eye.

From these data the actual weight of one such particle is readily found.

The first of these purposes may be accomplished as follows:

Let v represent any convenient volume (cc) of solution.

Let w represent the smallest weight (g) of substance which, dissolved in v, can be detected by the eye.

Then $\frac{w}{v}$ represents the weight in one unit volume.

Let us fix attention upon a definite portion of v. Suppose it to be in the form of a cube, and represent its volume (cc) by l^3, and let m represent the weight dissolved in this volume. It is evident that

$$m=\frac{w\,l^3}{v} \qquad (1)$$

By assigning known values to v and l^3, the value of w can be found by experiment, as will shortly appear, when m—the smallest actual weight of the solid in the cubical volume l^3—may be found.

We next proceed to ascertain the least number of particles into which the mass of m must be divided.

It may be fairly assumed that the particles are uniformly distributed in the solution; therefore, the number in the volume l^3 depends on the value of the uniform distance by which they are separated, and this distance is fixed in any case by the sensitivity of the eye.

We may represent by d the diameter of the smallest magnitude which the eye can perceive (we will suppose it to be a circular disk for a reason which will shortly appear). Then this value, d, would be the uniform distance, measured from center to center, of the particles in solution, provided, that the whole number required to absorb light enough to affect the eye, could be arranged in a single

layer instead of being uniformly distributed through the volume of solution. Let us conceive the whole number in the volume l^3 to be arranged in that way, they would form a layer whose area would be l^2. The number of particles along one side of this square would be represented by $\frac{l}{d}$, and hence the whole number in the layer would be $\frac{l^2}{d^2}$.

But since color is due to the absorption of luminous energy from white light, and since the absorptive power of a given mass is constant, this number of particles ($\frac{l^2}{d^2}$) would absorb the same amount of energy and affect the eye in the same degree whether arranged in a single layer, whose area is l^2, or distributed uniformly through a volume l^3, the area of whose section exposed to the eye is l^2, the same as before. Hence $\frac{l^2}{d^2}$ represents the smallest number of particles of the coloring matter which, uniformly distributed in the volume l^3, is required to affect the eye.

At this point we can bring our reasoning to the test of experiment. If the same number of particles will yield the same intensity of color whether arranged in a single layer with area l^2 or distributed through a volume l^3, we must be willing to admit that a given mass of coloring matter will produce the same intensity of color whether the volume of the solution be large or small, the only conditions being that the area of the solution exposed to the eye shall be unchanged, and the cross section of the vessel which contains it shall be uniform.

To test this by experiment, two exactly similar vessels were constructed. Each was eight inches high, with a cross section inside of two inches square. Their walls were of wood, with their inside surfaces painted dead black, while their bottoms were colorless plate glass. These vessels were supported, side by side, a little above

a sheet of white paper, by which the light was reflected upward through the plate glass bottoms.

1. Fifty cubic centimeters of pure water was placed in each vessel, and three drops of a solution of aniline green were added to the contents of one. The green tint imparted to the water in this vessel could be detected and its intensity judged by comparison with the pure water in the other, as they stood side by side, by looking down through the fluid upon the white paper below. Fifty cubic centimeters of water were then added to each vessel, and the tints again compared. No diminution of intensity could be detected in the green solution. Again 50 cc of water were added to both, and this was repeated until 5×50 cc had been added, each time with the same result. No variation in the intensity of color could be detected, although at last the same quantity of coloring matter was dissolved in 300 cc of water as was at first dissolved in only one-sixth of that volume.

2. The experiment was also varied as follows: Fifty cubic centimeters of water having been placed in each vessel, three drops of the aniline green solution were added to each instead of to one only as before. Pure water was then added to one only, and the colors of the two were compared as before after each addition. No variation could be detected; 250 cc in one and 50 cc in the other contained the same mass of aniline green and were colored with equal intensity.

These experiments confirm the inference that a given weight of coloring matter will produce color of the same intensity in any quantity of water, small or large, provided only that it is held in a vessel whose cross section has a uniform area. And therefore our expression, $\frac{l_2}{d^2}$, represents the least number of particles into which the mass m must be divided in order to impart color—the faintest that can be detected—to the volume l^3.

But, in equation (1) we already have, for the weight of this mass, the expression $\frac{l^3}{v.}.w.$ Dividing this by the number of particles, $\frac{l^2}{d^2}$, we have for the weight x of one particle,

$$x=\frac{w.\ l.\ d^2.}{v} \qquad (2)$$

In this expression the values of w, l and v depend on the nature of the coloring substance, the volume of water employed, and the dimensions of the unit volume employed. But d is a constant: it represents the limit of visibility—the smallest linear magnitude which the eye can perceive.

We have now to consider the most probable value of this factor.

Several estimates of this value have been made. Tait gives $\frac{1}{2000}$ inch as the smallest space which can be detected in reading the graduation of a vernier. Mayer has ingeniously shown[1] that the eye can detect a line drawn on a dull surface when its width is $\frac{1}{3000}$ inch, and that a black disc on a white surface can be detected when it measures $\frac{1}{300}$ inch in diameter. It is evident, from these different estimates, that the limit of visibility depends on the kind of magnitude which is observed.

Now the spaces between the particles in solution are certainly not lines. They are doubtless volumes. But as they lie before the eye they will present themselves as areas. They resemble Mayer's discs. But, inasmuch as we see the color of the solution by transmitted light, the eye is affected by the luminous energy that filters through them, so that these spaces should be regarded rather as luminous discs on a dark ground, whereas in Mayer's experiments the discs were black upon a ground of white. Whether this reversal of conditions would af-

1 "Minute Measurements in Modern Science."—*Sci. Am. Sup.*, Vol. III., page 879.

fect Mayer's value of the limit of visibility can be determined by experiment. The question is whether the eye can detect a smaller magnitude by the positive action of light received from it, than by the absence of action due to the loss of light which it intercepts.

Experiments to answer this question were instituted upon Mayer's [1] plan, considerably modified, as follows: Two blocks of wood, each eight inches square, were prepared with smooth surfaces made dead black. A disc of white paper, 4 mm in diameter, was cemented upon the face of one near its middle point. With this exception, the two blocks were as nearly as possible alike. Upon a large lawn, under a clear sky, the tests were made as follows: An assistant would hold the two blocks side by side while the observer, with his back toward the sun, would find a position so far in front of them that he could no more than detect the white spot with certainty, which was known by his being able to declare unerringly which block carried the spot, the blocks being changed in position, so that the observer could never know whether the disc was on the one at the right or left, above or below. The distance between the observer and the block was then measured. The vision of thirty young women, constituting a class in physics, was tested in this way, and the average distance was found to be 247 feet.

A disc 4 mm in diameter, at a distance of 247 feet, subtends the same angle of vision as one of 0.013495 mm at a distance of 10 inches—the average distance of distinct vision for small objects. Accordingly, for the average eye among the thirty individuals tested, the smallest white object that can be detected on a black ground is one whose diameter is 0.001349 cm. For such an eye, this is the value of d in the formula (2), which thus becomes

[1] "Minute Measurements in Modern Science."—*Sci. Am. Sup.*, Vol. III., page 879.

$$x = \frac{w.\ l.}{v} 0.0000018198 \qquad (3)$$

in which x is the value sought for, viz.: the actual weight of one of the particles into which a solid coloring substance may be divided when the eye is able to detect the faintest tint of its color in solution. The values of w, l and v are to be determined by experiment for each different substance: the "limit of visibility for mass" of these can then be calculated.

The experimental work has been carried out for several substances by means of an observation tank, rectangular in form, measuring about 20 cm long, about 15 cm high, and *exactly* 4 cm inside between its walls, which consist of colorless plate glass. A vertical partition between these walls divides the tank into two exactly similar compartments. A sheet of ground glass is fixed against the outside of one wall. A carefully measured volume of pure water is placed in one compartment and about the same quantity in the other. The tank is placed in front of a window looking northward, and a standard solution of the coloring matter is then slowly run from a burette into the compartment containing the known volume of water. The light from a clear northern sky, softened and diffused by the ground glass, traverses the liquid and enters the eye. With the pure water in the other compartment always in view for comparison, the observer is able to judge accurately when the water begins to show tint by the addition of the colored solution.

To illustrate the process and the calculations, the following description of the experiment with potassium chromate will suffice.

Thirty milligrams of the salt were carefully weighed, dissolved in pure water, and the solution made up to exactly 300 cc. Two hundred cubic centimeters of pure water were measured into one compartment of the observation tank, and the other compartment was also filled

with water to about the same level. The colored solution was then dropped from a burette, and thoroughly mixed with the water in one, until color could be fairly detected by contrast with the pure water beside it in the other. As a mean of five fairly concordant experiments, it was found that 6.9 cc were required by the 200 cc of water.

The calculations are as follows: The actual weight of the chromate, w, which was added to the 200 cc of water was $\frac{6.9}{300}\times 30$, which is 0.69 mg. or 0.00069 gram. The volume, v, of the solution containing this weight was2 06.9 cc. The length, l, of one side of the unit volume was the thickness of the stratum of fluid, or the distance between the walls of the tank, which was 4 cm. By substitution in formula (3), we have

$$x=\frac{0.00069\times 4\times 0.0000018198}{206.9}=0.00000000002427 \text{ gram.}$$

It may be remarked that, in the construction of the observation tank, the only measurement which needs to be known with exactness is the inside diameter from front to back. So long as the other dimensions remain constant, the same weight, w, of coloring substance, will impart the same tint to the solution, whatever the thickness, l, of the stratum of fluid may be, but the volume, v, of the solution will vary directly as the thickness. The actual distance, therefore, between the sides of the tank may have any convenient value, but it is essential that this value should be accurately known. The front area of the tank is reasonably small in order that all points may be before the eye with equal distinctness at the same moment.

The following results, all of which were obtained by the same observer, will still further illustrate the order of magnitudes reached by this method of testing the divisibility of coloring substances. No doubt eyes of different individuals would require some variation in the

significant figures in these values. Moreover, the same eye is not equally sensitive to all colors. Nevertheless, it is possible to arrive at a fair average "limit of visibility" *for mass*, which shall be as nearly correct as is the "limit of visibility" for extension, and which can be accepted as a fact of the same order of certainty as the fact embodied in the statement that ten inches is the "limit of distinct vision."

Mass Limits of Visibility.

	Gram.
Potassium Chromate	0.0000000000243
Potassium Dichromate	0.0000000000287
Potassium Permanganate	0.0000000000031
Potassium Ferricyanide	0.0000000000264
Potassium Ferrocyanide	0.0000000060744
Copper Sulphate	0.0000000036450
Malachite Green	0.00000000000049

APRIL 24, 1888. SIXTY-FOURTH REGULAR MEETING.

Charles N. Arnold, chairman presiding; eight members and eleven guests present.

Charles B. Warring, Ph.D., read the following paper:

REAL OR SUPPOSED CHANGES OF LATITUDE.

In Professor C. A. Young's address before A. A. A. S., at Philadelphia in 1884, he says, "The observations by Nyren at Pulkowa seem to show a slow, steady diminution of the latitude of that observatory during the last 25 years, of about 1″ in a century, as if the north pole was drifting away, and increasing its distance from Pulkowa about one foot in a year. The Greenwich and Paris observatories do not show any such result; but they are not conclusive on account of the difference of longitude, to say nothing of their inferior precision.

"The question is certainly a doubtful one ; but it is considered of so much importance that at a meeting of the Geodetic Society of Rome in 1883, a resolution was adopted recommending observations specially to settle it."

The above is from Nature, Vol. 30, page 502.

In the same journal, page 512, Mr. W. M. Flinders Petrie says : Only last year in the "*Pyramids and Temples of Gizeh*," I had noted (page 126) that the Greenwich observations did appear to show a change of the same amount and direction as stated by Prof. Young for Pulkowa.

Again, also in Nature, page 536, Mr. W. H. M. Christie says that there has been no sensible change of latitude at Greenwich (as found by observations of circumpolar stars) during the last forty-seven years.

He gives the result for the co-lat. for three periods of years :

1836–49	(14 years), mean	co-lat.	38°	31′	21.85.″
1851–65	(14 years) "	"	38°	31′	21.87.″
1866–83	(16 years) "	"	38°	31′	21.85.″

This would seem to settle the question that for these 44 years there had been no sensible change, the first and last being absolutely identical while the other group differ but $\frac{2}{100}$ of a second, an amount too small to be regarded at least at present.

But Mr. Petrie in the same vol. page 561, returns to the attack, and says : "It was from diagrams in Mr. Christie's paper that I drew the conclusions of a decrease of latitude." Mr. Petrie states that he drew his conclusions from Polaris alone, thinking that safer than stars having N. Polar declination since it is less affected by refraction.

He thinks it would be very remarkable if the great oceanic circulation should have a mean axis of motion so nearly coincident with that of the earth as not to pro-

duce $\frac{1}{100}$ of a second change in the Pole during half a century, the presumption would seem to be against such a fixity.

The question appears to be still an open one. Till it is settled that the latitude is changing, it would seem premature to speculate upon the cause of the movement, yet it may not be out of place to inquire what force exists that could produce such an effect.

Mr. Petrie, as quoted above, speaks of the influence of ocean currents, or, as he styles it, the great oceanic circulation. It is difficult to see how that could affect the latitude. Since every current has its opposite current, the eastward effect would be neutralized by the westward and the tendency to increase the latitude—if it exists—meets its counterpart in a tendency to diminish it.

It is well known that there are now and have always been movements in the earth's crust, caused by its cooling and contracting. The most evident effect of these now are the elevations and depressions going on at the present time in various parts of the world, as in Norway, Italy, the eastern coast of the United States, and elsewhere. There were horizontal movements in the past, shown by the folding of the strata, and by the sliding of the strata one over the other. The very uprisings are in general, due to lateral pressure and consequent movement. It is quite certain that from this cause every part of the earth's crust has at some time changed either its latitude, or longitude, and probably both. This cause is still active, hence it is more than probable that many places on the earth are slowly carried either towards or from the equator. If Pulkowa happens to be on a part of the earth's crust that is being pushed southward, say a foot in a year, then its change of latitude is easily accounted for. If Greenwich happens to be on a part of the crust where these forces are at present inactive, then

the almost absolute identity of the latitudes observed for the past 47 years, is also accounted for.

It is no new idea that the land surface of the globe is more unstable than that of the sea, but no one seems to have noticed that this fact had any bearing on those two important astronomical elements—the latitude and longitude.

If such local movements in latitude are really taking place, it will be comparatively easy to detect them since the latitude at any particular moment can be determined by observation to a very high degree of accuracy. With movements east or west, the problem is more difficult, but not impossible of solution, by the aid of the electric wires. With their assistance, it may hereafter be possible to determine the longitude, and hence the distance between places, with as much exactness as the latitude.

Such movements of the crust may explain discrepancies between longitudes of the same places at different epochs.

In short, the movements of the crust, introduces a new problem for the consideration of astronomers which in this time of delicate and refined methods, cannot safely be neglected.

THE GRAND CENTRE OF THE UNIVERSE.

Since gravitation is an all-pervading force, every atom tending to pull towards it every other atom, it would seem that the final result must be the destruction of all, the sun and its planets, the stars and their systems at last heaped together in one huge lifeless mass, the only vestige of the former arrangement being an eternal rotation on its own axis.

Science knows nothing to prevent such a catastrophe, except that force which now keeps the planets from fall-

ing into the sun, which, reaching down to the least, causes the water to stay in the pail which the school boy whirls around his head, and often spreads destruction by the bursting of a too rapidly revolving wheel.

This is the so-called "centrifugal force." Its philosophy I need not explain. It is sufficient for my purpose that all recognize it.

As all know, if the boy ceases to revolve the pail, its contents fall. If the wheel is at rest, the centrifugal force no longer acts. It is the result of motion around a centre, always coming with such movement.

Hence if our solar system and all its fellow systems are revolving around some grand center, the danger of all tumbling together like an ill-constructed arch is removed, and from unceasing movement comes stability.

The mind so revolts from any scheme in nature that seems to bear marks of imperfection that for this reason alone we would infer the reality of such a revolution.

There is, however, very satisfactory evidence that our own system is in rapid motion, and if ours, then others.

It has long been known as the result of careful measurements that the stars in the northern hemisphere are apparently separating while those in the southern seem to be drawing closer together. This cannot be due to a real movement for it is impossible to conceive of them as all moving at once, those in the north from a certain point, and those in the south towards a northern point 180° away, keeping, as it were, all in step like soldiers to music. If real, the movement would lack the uniformity it now exhibits.

The only explanation conceivable is that the two effects are optical illusions, the same in principle as when one stands on a train of cars in motion. If he is on the rear platform, the tracks, fences, trees and, in short everything that he passes, appear to approach each other. The track grows narrower, the fences move towards the

track, and the trees get closer together. All this is reversed to one on the engine. The track widens, the fences separate, the trees get wider apart as the train comes nearer to them. In neither case does he need to be told that nothing before, or behind the train is in motion, for he knows that the appearances are due to his own change of position. Reasoning identical in character, leads to the conclusion that the similar, apparent motions of the stars are due to a like cause, the rush through space of the sun with its train of planets.

For these reasons astronomers, long ago, came to the conclusion that the solar system is really moving, but positive proof was lacking. This has been supplied by the spectroscope. Aided by this instrument, in some respects the greatest of all the marvels of science, the astronomer can tell whether a luminous body is approaching the earth, or receding from it. It is even possible to say what the rate of approach is. Now when the spectroscope is turned to the heavens, it is found, in general, that the stars in the neighborhood of Hercules are approaching the earth, while those in the opposite part of the heavens are receding from it, exactly the effect which would result from solar motion.* It would be too absurd to suppose that all the northern stars are moving by one system as a river flows by a rock. It may therefore be admitted as beyond all reasonable question, that our system is in rapid motion, for it must be rapid motion to be detected by the spectroscope. Astronomers tell us that its rate is probably four miles a second.

Granted, then, that our system is in motion, the next and apparently necessary conclusion is that, somewhere in space, is a vast body, around which our system, probably in company with many others, revolves ; and then, that this body, with all its vast retinue of suns and

* See Newcomb's Astronomy.

planets, and in company with myriads of other systems, revolves around a yet greater body, until at last all find a controlling centre in the throne of God. To discover, from so complicated orbits, the position of the last grand centre is impossible, but to find the special centre around who our sun revolves, is not impossible. Given a few accurate observations taken at intervals of sufficient time, and astronomers could point to the exact spot, with greater accuracy than Leverrier pointed to the new planet. But unfortunately sufficient time means thousands of years. At present all that can be said with certainty is that the centre around which the sun revolves must by the laws of mechanics be ninety degrees from the point towards which it is moving.

This would place the central body in the direction of the Pleiades. And from this many have concluded that in that constellation is the central body whose attraction compels our sun with its planets to move around it.

There is something fascinating in the thought of system above system—central bodies dominating other central bodies of minor universes, and so rising step by step to the infinite central Power that called all into existence, and then ever after has held the central throne. This, however, is poetry, not fact. The Power is central, only as infinite space has its centre everywhere. To Him no spot is more central than another. In astronomy we must deal with facts, and relegate all else, however beautiful to the realm of fancy. There is no such great central body, for it would need to be millions of times larger than the sun, and many thousand times larger than all the systems controlled by it. A body so large, if heated like our sun, would appear even at that distance, immeasurably brighter than any star. To this it may be said that the fact that no such brilliant star is seen proves nothing because it may be a cold body like

the earth, and hence invisible since no reflected light could make it visible at that distance.

But there is another force not detected by the eye, yet omnipresent, that would render manifest the existence of such a body, I mean the attraction which it would exert upon neighboring stars. A world in the centre of the Pleiades, massive enough to control our sun and force it into an orbit in which it moves at the rate of four miles a second, would cause its immediate neighbors to revolve with such inconceivable velocity that only a few years would be necessary to make its existence evident. No such movements can be detected, hence no such body exists.

Many who will readily accept all that has thus far been said, will be slow to believe that the sun, if revolving at all, which is scarcely questionable, revolves around a mere mathematical point, something which has neither length, breadth nor thickness, but position only; and that this point is many thousand million miles distant from any star, or, in other words, that the sun is describing its inconceivably great orbit around a mere point in empty space; *i. e.*, around nothing at all. Yet such is the fact.

A very little consideration will serve to make this clear. Suppose, for a moment, that the only bodies in the universe were the sun and the nearest fixed star, and that these were of the same mass. Then, if they had a sufficient motion in a direction perpendicular to the line connecting them, they would move, not one around the other, but both around their common center of gravity, something as a pair of dumb bells, or two balls on opposite ends of a string. We have all seen how the string is drawn taut, and the two balls revolve around some point between them, a point whose distance from each is inversely as the masses.

In case of the star and our sun, if the masses were

equal, the central point would be half way between them, or, in case the star was our present nearest neighbor, the fixed central point would be out in space, 10,000,000,000,000 miles away from each.

If, however, the star were more massive than the sun, the point would be nearer to the star. If the latter were 1,000 times greater than our sun, the point would be 1,000 times nearer to it than to the sun, and yet distant from the former 20,000,000,000 miles, or more than 200 times farther from that enormous star than we are from the sun.

We may suppose another star added to this binary system, and, in similar manner, launched into space. The three bodies would revolve around the centre of gravity of the whole, and this, although the first and second would continue their rotation around their own mutual centre. We have an illustration of this in our solar system. The moon and earth revolve around their common centre, situated about 1,000 miles beneath the earth's surface, and on the line connecting the two, while at the same time they revolve with the sun around the common centre of the three.

If, to our triple system, other stars were added, the same law would hold good, although their movements would be so complicated as to defy the power of analysis.

If not too near each other, if, for example, they were at such distances as actually separate the stars, system upon system might revolve in harmonious but complicated movements, whose paths no finite intelligence could calculate, and yet, save for some interstellar resisting medium, they would go on forever.

From all this I conclude, that the stability of the universe is not due to the mastering power of some great central body, but to the action of part upon part combining all into one grand, orderly whole. The promise of permanency is found in the combined influence of the

primal impulse and of mutual gravitation. The former prevents the heavenly bodies from falling together, the latter keeps them from wandering off into the depths of space.

There is something so awe inspiring in the vastness of the heavenly bodies, their immense distances, and their enormous velocities, that one is in danger of being puffed up with the pride of intellect that the human mind can trace the orbits of the planets, weigh the sun itself, and take up the planets and their satellites, weigh them as in scales, foretell eclipses for centuries to come, and give, better than the actual observers, the time of beginning and ending of eclipses that occurred thousands of years ago. This all seems so great that we are tempted to ask, What is there that the human intellect cannot do? But when we look at the narrow bounds within which our calculus must work, then our pride comes down. The man who can trace a comet into the depths of almost infinite space, and tell where the goal is around which it wheels at the end of its course, and can announce the hour of its return, cannot tell where a falling leaf will strike the ground, nor trace a foot of its path. Everywhere we find problems to which our best solutions are but approximations, from which we have carefully eliminated the real difficulties.

The lesson is one of modesty and humility in the presence of infinitely profounder problems that meet us on every side.

Mr. Charles N. Arnold, chairman, presented his annual report, as follows:

Members of the Scientific Section of the Vassar Brothers Institute:

The meetings of the Scientific Section, Vassar Brothers Institute for the season of 1887–88 have been held with the customary regularity.

1887.

1. December 6. The chairman read some notes on "Woods and Gums."
2. December 20. Charles B. Warring, Ph.D., read a paper entitled "Miracles, Law, Evolution."

1888.

3. January 10. Professor William B. Dwight, Ph.D., lectured on (1) "Some Practical Suggestions as to the Preparation of Microscopic Sections of Fossils;" and (2) "New Devices in Machinery for Making Sections of Fossils and Minerals."
4. January 24. A letter from Dr. A. F. Woodward, of Brandon, Vermont, concerning the "Frozen Well," was read, and informally discussed by the Section.
   A brief but interesting paper on the "Relative Brilliancy of the Moon and the Electric Light" was presented by Charles B. Warring, Ph.D.
5. February 14. Professor William B. Dwight, Ph.D., gave a very interesting report of some recent discoveries by himself.
6. March 13. The "blizzard" summarily postponed the meeting of the Scientific Section.
7. March 27. Mr. Charles L. Bristol gave "Some Notes on Amateur Photography."
8. April 9. Professor Le Roy C. Cooley, Ph.D., read a paper on "Divisibility of Coloring Matter and Sensibility of the Eye."
9. April 24. Charles B. Warring, Ph.D., read a paper on "Some Terrestrial and Astronomical Matters."

A very interesting feature of these meetings have been the reports made by the Curator and the Librarian. The latter has brought to the notice of the Section many things in the "Proceedings" received by exchange with other societies which, otherwise, might have escaped the notice of members. The attendance has been about as large as usual.

C. N. ARNOLD,
Chairman.

The following gentlemen were elected officers of the Section for 1888–89:

Chairman, . . . MR. CHARLES L. BRISTOL,
Recording Secretary, . . MR. JAMES WINNE.

Curator of the Museum, Wm. G. Stevenson, M.D.
Librarian, . . . Mr. Charles N. Arnold.

NOVEMBER 27, 1888—SIXTY-FIFTH REGULAR MEETING.

No paper was read, and the meeting was of an informal character.

DECEMBER 18, 1888—SIXTY-SIXTH REGULAR MEETING.

W. G. Stevenson, chairman *pro tem.*, presiding.

Charles B. Warring, Ph.D., was elected chairman, *vice* Charles L. Bristol, removed from the city.

The following resolutions were adopted by the Section:

Resolved, That the Scientific Section petition the trustees of Vassar Brothers Institute to appropriate such sum of money as may be found necessary to properly catalogue the present library.

Resolved, That the Scientific Section petition the trustees of Vassar Brothers Institute to purchase for the use of the Institute a stereopticon adequate to the demonstration of scientific principles.

JANUARY 15, 1889—SIXTY-SEVENTH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; seven members and thirteen guests present.

The following paper was read by Professor W. B. Dwight:

"THE CAMBRIAN SYSTEM OF STRATA."

BY WILLIAM B. DWIGHT.

A very annoying confusion exists among geologists as to the classification and nomenclature of the lower sedimentary and metamorphosed rocks. Every geological field-worker in these strata encounters this difficulty

when he comes to make a report of his work. Whatever nomenclature he adopts is sure to meet with adverse criticism from many geologists.

This confusion has had two chief points of origin ; one in Europe, one in America. The trouble in Europe arose primarily out of the excellent and honest, yet partially conflicting work of the two great field-workers on English strata—Sedgwick and Murchison. A fair account of this is given in Geikie's "Text Book of Geology."*

Mr. Murchison began in 1831 the study of the Sub-Devonian fossiliferous strata, which, as a whole, he designated Silurian ; these he subsequently subdivided into Upper and Lower Silurian, with the dividing line between the unconformable Upper and Lower Llandovery groups. In continuing his researches through a long period of years, he gradually extended the Lower Silurian downward by new finds of fossils until he made it to embrace all the lower strata which were characterized by a "trilobitic and brachiopodous fauna." This was, in fact, claiming under his title all the lower strata known to be fossiliferous ; for all the strata presumably of sedimentary origin which were lower were considered and described as "barren slates and grits." It should here be noted that Murchison was preëminently a paleontologist, and was guided in his stratigraphic conclusions primarily by the character of the fossils collected.

His work was carried on chiefly along the borders of England and Wales.

Meanwhile, Sedgwick, with no less zeal, beginning at the other and lowest end of the problem, had been carrying on his work in Wales. The lowest and barren schists and grits of Wales he called Cambrian. So far, there was as yet no conflict with the Silurian of Murchison. But he soon extended his researches and the title

* Second Edition, 1885, page 650.

Cambrian upward into the higher fossiliferous rocks closely related to the barren Cambrian strata first studied. Still Sedgwick paid little attention to the paleontology in drawing his conclusions; he collected many fossils and laid them aside for future study, but he classified the rock on the general principles of comparative stratigraphy, somewhat independently of the paleontology. Thus he continued to work upward until he claimed as Cambrian strata not only the lowest "barren shales and grits" which Murchison had allowed him, but the higher strata up to the very base of the Upper Silurian. Thus Murchison's "Silurian" and Sedgwick's "Cambrian" overlapped each other in a confusion which has never since been resolved.*

The title "Primordial Zone," proposed by Barrande for certain of the lower strata did not contribute any relief to the situation.

A fair compromise has, however, been provisionally effected by some of the eminent British geologists by calling the entire mass of Sub-Devonian strata Silurian, and then dividing them into three members. The lowest member is called the Cambrian or Primordial Silurian, and is itself subdivided into two series, the lower comprising the Harlech, Longmynd and Menevian Groups, and the higher the Lingula Flags and, according to some authorities, the Tremadoc Slates also. The second member is called the Lower Silurian. Beginning where the Cambrian Silurian leaves off, it extends through the Lower Llandovery. The third member, called the Upper Silurian, beginning with the Upper Llandovery, extends to the Devonian.

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* For a more complete presentation of this topic see an article published by Professor James D. Dana since the reading of the present paper, entitled "Sedgwick and Murchison; Cambrian and Silurian," in the *American Journal of Science*, March, 1890.

The additional complication of American origin arose from the proposal by Professor Emmons of the name Taconic for strata supposed by him to be pre-Potsdam. This name, though not so conceived by Professor Emmons, would, nevertheless, be a synonym for the earlier names—Cambrian and Primordial—proposed by Sedgwick and Barrande. It has been, and still is, a sad source of contention among American, and to some extent, among European geologists. But it has been clearly proved that, as originally applied by Professor Emmons, it covered strata which are for the most part Lower Silurian. This title, therefore, only makes the former confusion "worse confounded," and it would better be entirely dropped from geological nomenclature.*

Many geologists in the United States have come to prefer the title Cambrian for the fossiliferous strata from the Potsdam or equivalent groups down; some would, however, for reasons which are certainly strong, include the Calciferous, as more allied to the Potsdam than to the strata of the Trenton period. It would appear that this use of the term Cambrian is the one which meets the approval of the United States Geological Survey.

If this plan is adopted, the next question would be as to the proper subdivision of the Cambrian strata. Mr. C. D. Walcott, the field geologist of the United States Geological Survey who is the chief expert in the study of these lower rocks, has recently proposed, and provisionally adopted, the following classification:

1. Upper Cambrian—Comprising the Potsdam Group, Tonto Group, &c.
2. Middle Cambrian—The Paradoxides horizon.

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* For a full presentation of the history of the Taconic discussions and researches, see the able paper by Professor James D. Dana entitled "A Brief History of Taconic Ideas," *American Journal of Science*, December, 1888.

3. Lower Cambrian—Known as the Georgia Group, the Olenellus Group, &c.; the strata containing Olenellus trilobites.

Until very recently, Mr. Walcott, and Mr. S. W. Ford, who is high authority in regard to Cambrian strata, had considered the "Georgia Group" to be the Middle Cambrian, and the Paradoxides beds the Lower Cambrian, in direct opposition to the views of the eminent Swedish authorities; but lately Mr. Walcott has discovered evidence which has compelled him for himself and others to reverse this opinion, and come into harmony with the Swedish views.

Under this subdivision of the Cambrian, each member is characterized by a prevailing trilobitic type—the Upper, by the Dicellocephalus; the Middle, by the Paradoxides; the Lower, by the Olenellus. These trilobitic names may be used as convenient designations for the respective subdivisions.

It must be noted, however, that while this is a convevient "working" subdivision, it is not at all certain, as yet, that it is in all respects a valid one; for it is not yet fully established that the Lower and Middle Cambrian are entirely distinct groups in chronological sequence. In fact, some indications have been found of the possible mingling of their fossils. It may prove that the difference in their fauna is one of sedimentation rather than of epoch. But until this should be proved, it seems more proper to give them the chronological order above indicated.

DISCOVERY OF FOSSILIFEROUS STRATA OF THE MIDDLE CAMBRIAN, AT STISSING, N. Y.

BY WILLIAM B. DWIGHT.

My field work in Dutchess County since the early part of 1886 has shown the presence of very extensive strata

of the Upper Cambrian, or Potsdam limestones, stretching through the western part of the county; also the occurrence of both limestone and quartzyte, containing fossils, of the Lower Cambrian or Olenellus horizon, which strata are apparently abundant, though confined to the flanks of the gneissoid ranges forming conspicuous features in some parts of the county.

Recent work enables me now to add another page to the Cambrian history of this vicinity, and a page of no small interest to geologists.

About one-third of a mile south of Stissing station, on the New York and Massachusetts Railroad, there is a low cut through a ledge of massive arenaceous limestones, alternating with calcareous shales. In July, 1887, fossils were found in some of the loose limestone débris in this cut. They were too imperfect to identify thoroughly, but were supposed to belong probably to the Potsdam. In the summer of 1888, since fossils proved to be exceedingly scarce throughout the northern part of the county in these limestones and calcareous shales, a more careful search was made in this cut. The greater part of the ledge yielded no results, but at last a thin fossiliferous layer was found near the ground, and very close to the railway track, not far from the southern end of the cut, and on the west side.

Roadmaster Joseph D. Neal very kindly put at my disposal a gang of the railroad employés to make the necessary excavation. Mr. Palmateer, who has charge of this "section," rendered very efficient service in conducting this work, and showed much skill in detecting fossils. The organisms discovered proved to be highly important in their stratigraphic relations, as well as interesting in themselves. They are all new and undescribed species, except one; but their character shows clearly that the rocks in this cut belong to the Middle Cambrian or Paradoxides horizon. This is the first time

that strata of this epoch have been identified in this State, with the single exception of the discovery, earlier in the same season, by Mr. C. D. Walcott of the United States Geological Survey, of fossils which he is "inclined to refer to the Paradoxides zone," in a locality in Washington County.

Fragments of these fossils abound in some of the thin layers at Stissing; but good specimens are very rare, and have been secured only by the breaking up of a great deal of the rock into small fragments. Some of these specimens exhibit the details of the structure quite perfectly.

The list of the fossils of the Paradoxides zone obtained in excavating the rock in this locality is as follows:

1. Hyolithes Billingsi?—Plate —, fig. 1.—About half a dozen poorly preserved tubes of a fossil, referred to this species, have been found here. They are from eight to twelve millimeters long, and from three to four in diameter at the aperture. This fossil, even if the species were certain, would not determine the exact epoch, as it has considerable range of occurrence.

2. Leperditia ebenina—Plate —, figs. 2, 3 and 4.—New species. This ostracoid has a very characteristic jet black, shining, sub-elliptical carapace, about eight millimeters in length. The exterior surface is quite peculiarly ornamented in this way; the entire border of each valve, to the width of about two millimeters in the larger specimens, is covered with extremely minute, *contiguous* pits, about 100 to 150 to a square millimeter. All the surface within this border is covered with much larger, *separated* pits, the interspaces being at least as wide as the pits. The disposition of these pits is quite irregular, but they average about 15 or 20 to the square millimeter. There is also a linear marginal groove along the ventral border. The central portions of the internal surfaces of the valves are covered with scattered, well-

defined tubercles, corresponding, apparently, with the scattered pits of the external surface.

3. Kutorgina Stissingensis—Plate —, figs. 5, 6, 7 and 8.—New species. Shell black, phosphatic; width slightly greater than the length, and, in most of the specimens collected, is about eight millimeters. General shape semi-circular. The ventral valve has an elevated, pointed beak, from which the surface slopes down regularly toward the lateral margins, while in sloping down to the front margin it sometimes becomes concave centrally to the shell. Along the cardinal border of this valve, the shell is suddenly deflexed toward the hinge line, making a false area, separated centrally into two parts by a vacant deltidial space.

The dorsal valve is depressed and nearly flat, with a low beak.

The surfaces of both valves are covered with very fine, sharp, concentric ridges, traversed by striæ scarcely visible to the naked eye. These surface markings are inclined to run into minute undulations, and the effect of this ornamentation, as viewed through a strong triplet, is often that of lovely basketwork.

This exquisitely beautiful shell is allied to the well-known species, Kutorgina Labradorica. Imperfect fragments of it abound in these layers, and would easily be taken for fragments of Lingulepis pinniformis of the Potsdam. Close inspection will, however, reveal this distinction; the concentric ridges or laminæ of L. pinniformis (at least as exhibited in Dutchess County, N. Y.), are feebly defined, when magnified, and often run together obscurely; while those of Kutorgina Stissingensis, as viewed with a strong triplet, are deeply cut, and generally individualized with exquisite perfection.

4. Olenoides Stissingensis.—Plate —, figs 9–15.—New species. This trilobite is very important in determining the epoch of these strata, since it belongs to a genus

which is believed to be characteristic of Middle Cambrian strata. The body is elongate ovate, a little over three centimeters long in the only entire specimen found. The head is large, semi-circular, with large elongate eyes, The glabella is elongate, its length about one and two-thirds times its least width. Dorsal furrow well defined, but not deep. Glabellar furrows, three or four. Occipital furrow strongly defined at its outer extremities, but narrow and shallow at the center. Occipital ring triangular; lower than the glabella; very broad centrally, narrowing rapidly toward the lateral terminations; it terminates posteriorly in an obtuse point. The free cheeks are triangular, and have a long spine. Hypostoma triangular, well rounded anteriorly, with a well-marked annulation near the small posterior end.

The thorax contains eight segments. A linear furrow, deeply impressed, passes from one posterior angle of each axial segment to the other, traversing the central point of the segment in a circular line, convex anteriorly. There is a tubercle, or perhaps the base of a spine, on each axial segment, just to the rear of its center.

The pleural segments are depressed, convex, and extend at nearly right angles to the axis; they are narrowed from the anterior side, and prolonged into flat, acute, recurved spines, with broad, contiguous bases.

The pygidium is of moderate size, triangular; axis strong, elevated, obconical, with at least two transverse furrows anteriorly. The lateral lobes consist of an inner depressed convex portion, traversed by two or three oblique furrows, and a perfectly flat and moderately broad margin, from which three flat and acute spines extend backward.

This interesting trilobite is closely related to Olenoides Nevadensis, Meek; but it differs strongly in the more slender thoracic axis, the shape and surface markings of the axial thoracic segments, the broad, flat, pleural thor-

acic spinous processes, and the different structure of the pygidium.

It is very desirable that other fossiliferous localities of this zone should be found in the county; but much search has so far failed to reveal any such in the immense displays of calcareous shales associated with limestones in the northern part of Dutchess County.

EXPLANATION OF PLATE.

MIDDLE CAMBRIAN FOSSILS FROM STISSING, N. Y.

Natural size, except where otherwise noted. All are from the calcareous shales, except those represented by figs. 5, 6 and 15, which are from the limestones.

Fig. 1. *Hyolithes Billingsi* (?), cast of interior, showing three or four slight annulations; the anterior one more prominent than the others.

Fig. 2. *Leperditia ebenina*, n. sp., enlarged to 2 diameters; fragment of (right?) valve, showing the line of the hinge, and a sloping dorsal angle, also the outer belt of minute contiguous pits, and the inner tract of larger separated pits. The ornamentation indicates that the complete carapace must have been at least one-sixth longer than the fragment.

Fig. 3. *L. ebenina* enlarged to 2 diameters: lacking the cardinal margin; showing perfectly the peculiar surface-pitting, and the ventral furrow.

Fig. 4. *L. ebenina*, interior view of a central fragment of a valve; showing the separated tubercles, corresponding to the separated pits of the central exterior.

Enlarged to 2 diameters.

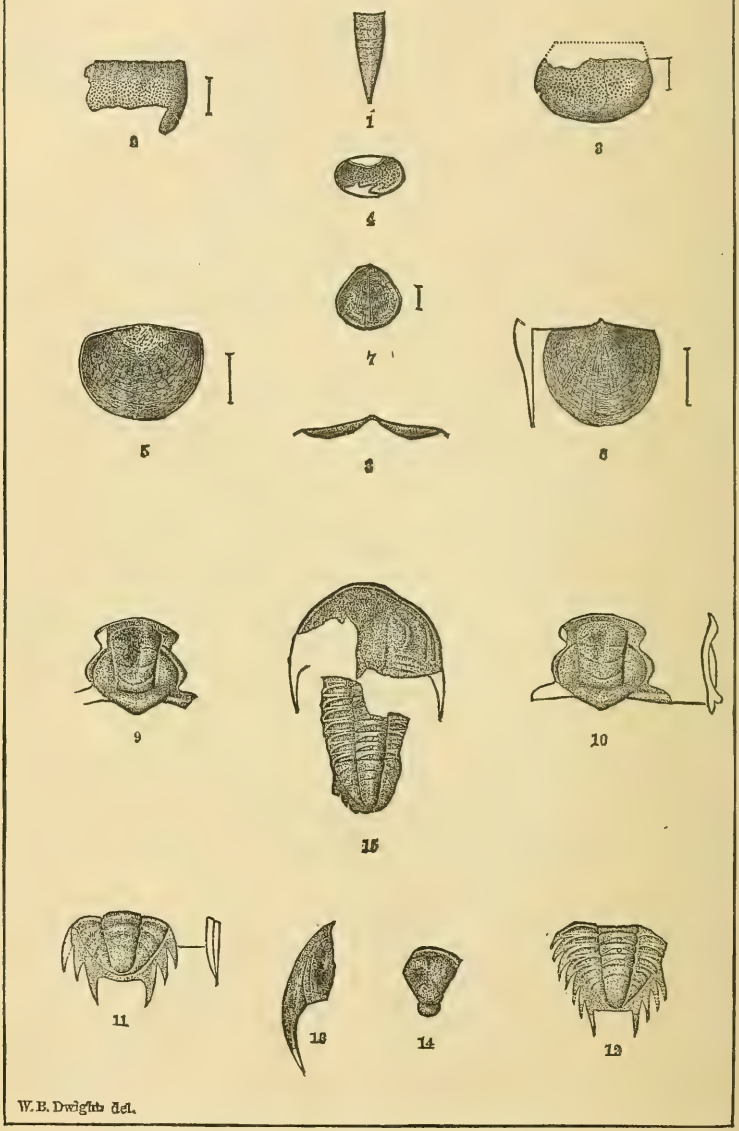
Fig. 5. *Kutorgina Stissingensis*, n. sp., enlarged to 2 diameters; a natural cast of the dorsal valve.

Fig. 6. *K. Stissingensis;* enlarged to 2 diameters; ventral valve; with a side view of the elevation.

Fig. 7. Gutta percha cast of a natural impression of the interior of the unbonal region of a ventral valve, referred to *K. Stissingensis;* showing a medial septum from which fine striæ diverge, and muscular impressions, enlarged to 3 diameters.

Fig. 8. *K. Stissingensis*, cardinal view; showing false area, deltidial opening and the rounded edge between the false area and the main surface of the valve; enlarged to 2 diameters.

PLATE.



MIDDLE CAMBRIAN FOSSILS FROM STISSING, N. Y.

Figs. 9 and 10. *Olenoides Stissingensis*, n. sp.; the glabella. Fig. 9 from an artificial cast, Fig. 10 with a side view of the elevation.
Fig. 11. *O. Stissingensis*, pygidium.
Fig. 12. *O. Stissingensis*, pygidium, with four attached thoracic segments.
Fig. 13. A free cheek, associated with *O. Stissingensis*.
Fig. 14. Hypostoma of *O. Stissingensis*.
Fig. 15. *O. Stissingensis*, full length, showing eight thoracic segments; details of the glabella obliterated or distorted by compression.

Dr. Stevenson nominated for membership of the section, Dr. David B. Ward.

JANUARY 29, 1889. SIXTY-EIGHTH REGULAR MEETING.

Dr. Charles B. Warring, chairman, presiding; the curator reported that a catalogue of the Herbarium was being prepared. The librarian reported the reception from Colorado School of Mines, its quarterly publication; also the publication of the Society of Natural History, of Glasgow; also a communication from a society of Naturalists at Kieo, asking an exchange of publications.

Dr. David B. Ward, Dr. J. E. Hoffman, and Mr. Gilbert Van Ingen, were unanimously elected members of the section.

FEBRUARY 12, 1889. SIXTY-NINTH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; ten members and five guests present.

The chairman, Dr. Warring, read the following papers, which were discussed by the members present:

SOME CURIOUS SIMPLE EQUATIONS.

It has long been my custom to form, extempore, exercises for my classes in Algebra. Once throwing to-

gether, almost haphazzard, three equations, apparently independent of each other, I found that they would not solve. The following is substantially the set of equations which I happened on :

(No. 1.)
$$\begin{aligned} x+2y+3z&=6\\ x+4y+7z&=12\\ x+3y+5z&=9 \end{aligned}$$

These are apparently independent but on trial it will be found that when reduced they give two identical equations.

As some other teacher may happen on a similar set and be perplexed, for, so far as I know, no one has ever noticed, much less explained them, I propose in this paper to supply the deficiency, first giving by way of illustration several sets possessing the same curious properties.

(No. 2.)
$$\begin{aligned} x-y-w&=a\\ 2x-5y-6w&=b\\ 18x-3y-2w&=c \end{aligned}$$

(No. 3.)
$$\begin{aligned} x+y-w&=r\\ x+\frac{w}{3}&=s\\ y-\frac{4w}{3}&=t \end{aligned}$$

(No. 4.)
$$\begin{aligned} x-y+3z-w&=a\\ x-\frac{3y}{2}+5z-\frac{7w}{4}&=b\\ x+\frac{y}{2}-3z+\frac{5w}{4}&=c\\ 4x-\frac{3y}{2}+2z-\frac{w}{4}&=d \end{aligned}$$

These will suffice. They can be indefinitely increased, and any number of letters may be used.

It will be seen from the number of such equations that can be made that it may easily occur to a teacher in giving examples off-hand, to hit upon some of this kind.

On examination it will be seen that the co-efficients in each equation of No. 1 are in arithmetical progression.

The same is true of each of the other sets but the progression is not apparent. It was obtained thus: I wrote

$$x+2(\frac{-y}{2})+3(\frac{-w}{3})=m$$

$$x+5(\frac{-y}{2})+9(\frac{-w}{3})=q$$

$$3x+(\frac{-y}{2})-1(\frac{-w}{3})=p$$

Here the co-efficients are in arithmetic progression.

We have only to remove the brackets and clear of fractions and then we have set No. 2.

As to No. 3: I wrote,

$$x+2(\frac{y}{2})\times 3(\frac{-w}{3})=a$$

$$x+0(\frac{y}{2})-1(\frac{-w}{3})=q$$

$$0(x)-2(\frac{y}{2})-4(\frac{-w}{3})=p$$

and by the same process, I get that set.

No. 4 was formed in like manner. In no case can equations, the co-efficients of whose terms form, in each equation, an arithmetical progression be solved.

Proof.

Let
$$x+(1+a)y+(1+2a)z=q$$
$$x+(1+na)y+(1+2na)z=q'$$
$$x+(1+n'a)y+(1+2n'a)z=q''$$

be three general equations the co-efficients of which are in arithmetical progressions.

By subtraction we have

$$(na-a)y+(2na-2a)z=q'-q$$
$$(n'a-na)y+(2n'a-2na)z=q''-q'$$

Dividing by the co-efficients of y and z, we have

$$y+2z=\frac{q'-q}{an-a}$$

$$y+2z=\frac{q''-q'}{an'-a}$$

the left hand members of which are identical. Since a is any difference, and na and $n'a$ any multiples of a, all equations will come out the same way.—Q. E. D.

It can be shown in the same way that four, or any number of equations, whose coefficients are in arithmetical progression, are indeterminate.

IMAGINARY QUANTITIES—THEIR PHILOSOPHY.

BY C. B. WARRING, Ph.D.

An imaginary quantity is defined to be the even root of a negative quantity, for example, $\sqrt{-a}$; and a root of a number is a number which, multiplied by itself, will produce that number. It follows from the idea of opposition, which is the fundamental difference between + and —, that while $+\times+$ produces +, so also $-\times-$ produces +.

I may write on the board the expression $\sqrt{-1}$. But since $+1\times+1=+1$ and $-1\times-1=+1$, there can be no such number as the square root of —1, *i. e.*, no number which, multiplied by itself, will produce —1. The mind is baffled when it endeavors to form a conception of such a number. For this reason, such expressions have very justly been called impossible quantities, and, very incorrectly, imaginary quantities, for the imagination refuses to recognize them. A better name would be pseuds (from ψεῦδος). Their claim to this title will appear later on.

Yet certain very real results are obtained by their use. If, *e. g.*, I multiply $(6-5\sqrt{-1})$ by $(6+5\sqrt{-1})$, I get 61, a result from which both imaginaries have vanished.

And besides this, some mathematical truths otherwise not easily demonstrated are obtained by their help. The following example, taken in substance from the ninth edition of the Encyclopedia Britannica, will suffice for an illustration :

It is required to demonstrate that the product of the sum of two squares is itself the sum of two other squares. In other words, it is required to prove that $(a^2+b^2)+(c^2+d^2)$ equals the sum of two squares.

(1). Since $(x+y\sqrt{-1})(x-y\sqrt{-1})=x^2+y^2$, we may assume

(2). $a^2+b^2=(a+b\sqrt{-1})(a-b\sqrt{-1})$.

(3). $c^2+d^2=(c+d\sqrt{-1})(c-d\sqrt{-1})$.

$(2)\times(3)=(a+b\sqrt{-1})(c-d\sqrt{-1})(a-b\sqrt{-1})(c+d\sqrt{-1})$

Or, $(ac-ad\sqrt{-1}+bc\sqrt{-1}+bd)\ (ac+ad\sqrt{-1}-bc\sqrt{-1}+bd)$.

Or, $(ac+bd+bc-ad\sqrt{-1})\ (ac+bd-bc-ad\sqrt{-1})$.

If we put $ac+bd=A$,

and $bc-ad=B$, we have

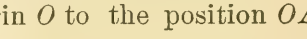
$$(2)\times(3)=(A+B\sqrt{-1})\ (A-B\sqrt{-1})$$

or, $(a^2+b^2)\ (c^2+d^2)=A^2+B^2$. Q. E. D.

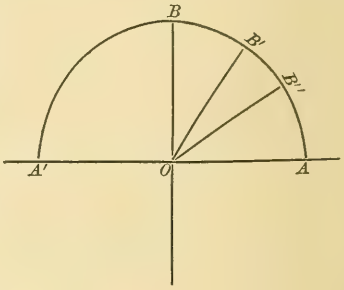
What then can be done to reconcile our minds to the apparently illogical result that, from unreal, impossible premises, come real and true conclusions ?

To aid in doing this, it has been attempted to represent the impossible (or "imaginary") quantities graphically. It has been said that Multiplication, in its broad sense, is not merely, as in Arithmetic, a process of repeating the Multiplicand, but, in algebra, it includes another idea, that of direction, and multiplying by —1 reverses the direction of the multiplicand, if this be represented by a line. Starting from a certain point called the origin, we agree that all toward the right shall be called positive, and all toward the left negative. Now, if we have one or more + units, *i. e.*, to the right, and multiply

by, *e. g.*, —1, the effect, it is said, is to revolve the line OB about the origin O to the position OA', OA describ-



ing a semi-circle on AA', and implying that OA, when multiplied by —1, passes in succession through all intermediate positions. Hence, as $-1=\sqrt{-1}\times\sqrt{-1}$, or, in other words, as there are here two equal factors, one of them will cause the line to rotate half way, or become perpendicular to AA'.



This line, OB, is said to be the graphical representation of a unit $\times\sqrt{-1}$, or simply $\sqrt{-1}$.

With perfect consistency, it is said $1\times\sqrt[3]{-1}$ is represented by OB', AOB' being $\frac{1}{3}$ of 180°; and $1\times\sqrt[5]{-1}$, by OB'', the angle AOB'' being $\frac{1}{5}$ of 180°, and so on.

But, since $\sqrt[3]{-1}=-1$, the result of multiplying OA by either of them should be the same, and, therefore, $OA\times\sqrt[3]{-1}$ should $=OA'$, or $-OA$.

In this graphic representation it is assumed that, in changing from + to –, OA, describes a semi-circumference about O, so that it has at one time the position OB, halfway between + and –. In reality, the change is usually made in a different way. A man walking east does not go west by any such circuitous route. A point in a chord vibrating under a single impulse, goes back and forth in a straight line, its motion becoming alternately + and –. If 100 feet east is changed to 100 west, the result is attained, not necessarily by revolving my tape around the fixed point, or "origin," but rather by sliding it by that point while keeping it parallel to itself.

Again, it is an objection to such a representation of im-

possible, or imaginary quantities that it gives a line which is neither larger, nor smaller, nor equal to certain other lines in the same diagram. It is true that such a relation, or rather such a lack of relation, can be predicated of *e. g.*, color and weight, or music and painting, but we do not attempt any graphical representation which brings both into one diagram.

Whatever advantages there may be in this mode of representing imaginary quantities, it explains nothing, for it is itself founded on the postulate that $\sqrt{-1}\times\sqrt{-1}=$ the actual quantity -1, and it is in this that the real difficulty lies. How is it that out of quantities that do not exist, real results arise?

Thus, *e. g.*, if I multiply $5+4\sqrt{-1}$ by $5-4\sqrt{-1}$, I get 41; or, omitting the real number, I multiply say $-\sqrt{-16}$ by $+\sqrt{-4}$, I get the real and positive number 8. I take something which does not exist, and multiply it by a number that does not exist, and I get something which does exist.

I think the explanation of these results lies in the principle that one false supposition may be corrected by another. As in grammar, we say two negatives make an affirmative, so in mathematics two suppositions, both false, may give a truth. One falsehood in our reasoning will vitiate our conclusion, but two may neutralize each other. If they do, then our conclusion will be true. If, for instance, I say that I stepped this evening from my office across the Atlantic into London, the conclusion would be that I am now in London, a false one. But if I add that I stepped from there into this, our Institute, then the logical conclusion would be that I came this evening from my office to this place, and this is the truth. I may put into my reasoning as many of these misstatements as I please; and if, in mathematical phrase, their sum is zero, my conclusion will be true.

If I write $5+2\sqrt{-1}$, it cannot equal any real number. If I write it equal to something, the statement is absurd:

$$\text{Whether I write } 5+2\sqrt{-1}=\sqrt{29}$$

$$\text{or} \qquad 5-2\sqrt{-1}=\sqrt{29}$$

each is a mathematical falsehood. But let me combine the two by multiplication, the imaginaries disappear, and I have the truth, $29=29$.

Untruths need to be handled carefully to get truth from their combination. If I had added the two pseudo-equations together, or had subtracted one from the other, the result would be false, for 10 does not equal $2\sqrt{29}$, nor does $4\sqrt{-1}=0$. If I take impossible quantities and perform upon them an impossible operation, the result may be a real, and, consequently, a true one. It therefore need excite no surprise that in the hand of a master such expressions as $\sqrt{-1}$ or $\sqrt{-x^{a\sqrt{-1}}}$ should yield real quantities.

FEBRUARY 26, 1889—SEVENTIETH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; twelve members and fifteen guests present.

Professor William B. Dwight, Ph.D., read a paper on "Glacial Phenomena."

This paper called attention to some of the more recent views of glacial phenomena, founded upon the latest observations.

Two points of special importance were presented:

1. The fact that Canadian geologists have been generally inclined to assign the phenomena of glaciation, as observed by them, to floating ice; pointing, for instance, to similar phenomena annually occurring in the mighty spring flood along the banks of the St. Lawrence River. On the other hand, the geologists of the United States

almost universally attribute the glacial phenomena of this country to glacier movements.

These apparently opposing views are now likely to find their reconcilement in the probability that in the United States the phenomena were chiefly due to glaciers, while in Canada there is reason to believe that icebergs or ice-fields, floating on great bays or rivers, may have combined with the great glaciers, which were undoubtedly present, to produce the visible phenomena of glaciation. Thus it is supposed that a great gulf of the Arctic ocean may have extended southward a long distance, up the basin of the Mackenzie River, carrying icebergs which produced the glacial phenomena of a large part of that region. (See "Glaciers and Glacial Radiants," by Dr. E. W. Claypole, in the *American Geologist*, February, 1889, page 83.)

2. The supposition that there was an enormously thick ice-cap covering the entire polar regions and extending south well into temperate latitudes, which is the basis of "Croll's theory," is becoming pretty thoroughly dissipated by recent investigations. It seems now highly probable that there was not even a uniformly thick sheet of ice stretched across the northern part of the North American continent from Labrador to Alaska, producing the embossing and striations everywhere visible in the high latitudes of the United States and in Canada. For it is now well understood that icebergs cannot be formed except from glaciers; that glaciers cannot be formed in great and continuous ice masses without the combination of heavy precipitation of snow, with the right conditions of enduring cold, and also of a difference between the cold and moisture of winter and of summer. Heavy precipitation of snow can occur only in regions of great moisture; and where the maxima of moisture exist with the other conditions, there will be the maximum thickness of ice-sheets, and *vice versa*.

It is then evident that the heavy falls of snow will be near the borders of the boreal portions of the continent, while the interior parts will be thinly covered. Moreover, the elevated borders are, by their elevation, better fitted to be the locus of thick ice-sheets than the low hills of the interior basins.

There is found to be a close correspondence between these principles and the facts shown by the glacial phenomena observed. There appear to be two great radiant centres of enormous glacial deposition in northern Canada and the United States—one in the region around Hudson Bay, the other in Alaska. From these the great glacier movements radiated, while to the north of the central portion of North America the glacial sheet was thin, and at intervals, perhaps, entirely intermitted. Greenland has, of course, always been a "glacial radiant." In Europe, the glacial radiant for England, Germany and Russia was the Scandinavian peninsula. There is no reason to suppose that any thick sheet of ice existed continuously, and at great elevation, in the insular or archipelagic portions of the Arctic regions. For a full discussion of this interesting subject, reference is made to the paper of Dr. Claypole, above cited.

It is estimated by Professor W. Upham that, in the culmination of the two principal glacial epochs, the thickness of the ice-sheet on the Laurentian highlands, and in the basin of James Bay, and over the south part of Hudson Bay, was from 10,000 to 12,000 feet.

MARCH 12, 1889—SEVENTY-FIRST REGULAR MEETING.

The meeting was of a social and informal character, no paper being presented.

MARCH 26, 1889—SEVENTY-SECOND REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; eight members and several guests present.

Professor L. C. Cooley, Ph.D., addressed the Section on "Illustrations of Simple Harmonic Motion."

The subject was fully illustrated by a series of experiments and by the lantern.

APRIL 9, 1889—SEVENTY-THIRD REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding.

Mr. Edward Elsworth read a paper on

THE PROGRESS OF PHOTOGRAPHY.

So common has the camera become ; so familiar are its mechanical construction and operations that thousands of its votaries doubtless forget, if they ever knew, that photography is the outcome of a long line of baffling scientific investigations.

"Any one can take pictures," advertises an enterprising manufacturer of cameras. "All that is necessary is to press a button. We do all the rest."

So there are many who never seek to penetrate the mysterious arena of the dark room, and scarcely stop to think how a photograph is made.

Perhaps no art has ever been so thoroughly popularized as the art of photography. But photography is more than a popular art. It is the legitimate child of science. It owes its development to years of patient labor and study of the subtle properties of light and its action upon sensitive surfaces.

Art has popularized it, and established its commercial value ; but its victories belong to the laboratory, and its future depends upon the experiments and investigation of the chemist.

The discoloring action of rays of light upon different substances has been the subject of observation for many centuries, but only within the last fifty years has experimental science demonstrated and explained the chemical reactions produced by light.

The most ancient references to the subject may be suggested by the facts that in the British Museum there is exhibited an object resembling a lens, which is said to have been found among the ruins of the ancient city of Nineveh ; and that Pliny recorded as a result of his observation that yellow wax was bleached by exposure to sunlight.

Fabricius, about the middle of the sixteenth century, searching for gold, found that the lunar cornea, or horn-silver, could be prepared by adding a solution of common salt to a solution of silver nitrate, and he was surprised at, and recorded the fact that this white compound turned quickly black when exposed to the sunlight.

No thought of utilizing this discovery seems to have occurred to him.

In 1727, Schultze obtained *copies* of writing, by writing first upon a paper whose surface had been prepared by chalk and nitrate of silver. It was found that the sun's rays passing through comparatively transparent paper blackened the surface of the prepared paper beneath, except where intercepted by the ink forming the letters, producing a white copy upon a black background. But it does not appear that Schultze made any attempt to fix the result of his experiment, or even put it to any practical use, and the experiment stands as an isolated one, yielding no fruit for half a century at least.

It remained for the distinguished chemist, Scheele, of Straslund, to first investigate and, in a measure, demonstrate, the chemical effect of the spectrum, thus stimulating the long line of investigation which led to the discovery of photography.

He discovered the facts, in 1777, that the different colored rays of light exercised an unequal effect upon the salts of silver; that silver chloride was darkened by the violet or blue rays in much less time than by the yellow or red rays. He also first discovered the cause of the darkening, viz.: that the effect of white light upon chloride of silver is to decompose it. This he accomplished by exposing chloride of silver under water, pouring off the water, and then adding a little nitrate of silver, whereby chloride of silver was again produced; proving that a decomposition of the former substance had taken place, and that chlorine had been dissolved in the water.

The conclusions of Scheele were disputed by some, even the distinguished Count Rumford maintaining that the active agent was *heat*, and not *light*. But the main results of Scheele's experiments are not disputed to-day. Still no one appeared to conceive any practical use to which the discovery of Scheele could be put. Although for more than a century suggestions had appeared along the way, they have waited until almost the last half of the nineteenth century to be developed into practical media for utilizing the knowledge of the philosophers that sunlight will print an image upon a surface coated with the salts of silver.

The camera had already been invented, and, by means of a lens, forms of which had been constructed ages before, beautiful pictures of scenery had been reflected upon the white walls of a chamber; and the secret door of photography seems almost to have been unlocked by a Frenchman, de la Roche, in 1760, in a book called "Giphantie," wherein he transports his hero to a strange land, where he is shown the method by which the native genii produce pictures. "You know," he writes, "that rays of light reflected from different bodies form pictures, paint the image reflected on all polished surfaces; for example, on the retina of the eye, on water and on

glass. The spirits have sought to fix these floating images; they have made a subtle matter by means of which a picture is formed in the twinkling of an eye. They coat a piece of canvas with this matter, and place it in front of the object to be taken. The first effect of this cloth is similar to that of a mirror, but, by means of its viscous nature, the prepared canvas, as is not the case with the mirror, retains a *fac simile* of the image. The mirror represents images faithfully, but retains none. Our canvas reflects them no less faithfully, but retains them all. This impression of the image is instantaneous. The canvas is then removed and deposited in a dark place. An hour later the impression is dry, and you have a picture in that no art can imitate its truthfulness."

This was, of course, several years before Scheele's experiments, and was purely the creation of de la Roche's imagination, yet it reads to-day like a remarkable prophecy, and surely may have stimulated the efforts of those who ultimately solved the problems of sun printing.

The early years of the nineteenth century were marked by the experiments of Wedgwood (one of the great English doctors) and of Sir Humphrey Davy, both of whom endeavored to secure the pictures formed within a camera, but without success. Davy discovered that chloride of silver was much more sensitive than the nitrate, and by using the concentrated light of the solar microscope he obtained images of small objects upon paper which had been coated with chloride of silver.

How to secure these images presented a new problem, which Davy did not live to solve. He wrote: "Nothing but a method of preventing the unshaded parts of the delineations from being colored by exposure to the day is wanting to render this process as useful as it is elegant."

The experiments of Nicéphore Niepce, with the bitumen process of contact printing, and the action of light upon silver, bitumen and other substances, are inter-

esting, only in their relation to the agreement and partnership which in 1829, was consummated between Niepce and Daguerre. Daguerre, the distinguished Frenchman, whose name is connected with the first successful effort to permanently fix the photographic image, was born near Paris in 1787. In his early manhood he became a scene painter, and frequently employing the ordinary camera obscura, he became inspired by the beauty and perfection of the pictures produced thereby, to attempt the discovery of some method by which they could be permanently retained. He was without any scientific training, and appears to have passed at least five years without accomplishing anything in the desired direction, except the production of a camera fitted with some lenses which he purchased from a well-known optician.

Hearing of Niepce in 1826, he attempted a correspondence with him, which met with but curt response; but in 1829, the two met in Paris and a friendly intercourse soon ripened into a copartnership, which continued until Niepce's death, and thereafter with the latter's son Isador.

For ten years Daguerre struggled, his ordinary business neglected, subjected to suspicion of insanity, striving ever, without much method perhaps, but with a vast amount of patience and perseverance, to discover some way to capture the fleeting images formed within his camera.

In 1838 he proclaimed success, and showing the results of his process to the eminent scientist, Arago, Arago was so delighted that he endorsed the discovery in the highest degree, and it was upon his recommendation that the French Government conferred upon Daguerre a life pension of six thousand francs, and upon Isador Niepce, a like pension of four thousand francs.

The condition of these pensions was that the discovery should be given to the world.

The success of Daguerre's and Niepce's efforts depended chiefly upon an accident.

Daguerre used the same materials which had been used by Wedgwood and Davy, viz., chloride and nitrate of silver upon paper, with no better result. Niepce sometimes used metal plates coated with silver and employed iodine to darken the exposed portions. Using vapor of iodine and silver coated plates. Daguerre found that the iodide of silver formed by exposing the plates to the vapor of iodine was sensitive to light, and that faint images of brightly illuminated objects became visible after an exposure of two or three hours. Leaving one day what he supposed was an underexposed plate, in a dark cupboard, he was surprised on the following morning to find thereon a distinct and perfect image. He repeated the experiment again and again, always with the same result.

By a process of elimination he ultimately discovered that the development of the image was due to the fact that the plates had been exposed while in the cupboard to the vapor escaping from a broken vessel of mercury.

Daguerre at once utilized his fortunate discovery by placing his exposed plates over a dish of warm mercury, the vapor from which acted upon the iodized silver in exact proportion to the intensity of the light, by which each part of the surface of the plates had been affected.

The result was a perfect picture, but now came again the problem which had confronted Davy. Some portions of the silver iodide had not been acted upon at all, and in a short time, exposure to the light must darken these and destroy the picture. It became necessary to remove from the plate all of the iodide of silver which had not been affected by the light.

Daguerre at first used a strong solution of common

salt for this purpose, but in 1839, Sir John Herschel called his attention to the superior qualities of hyposulphite of soda as a solvent of silver salts, and Daguerre immediately adopted it.

It has ever since been used for the same purpose, and the hypo bath is an indispensable adjunct of every photographer's dark room at the present day.

Such was the birth of photography !

Thanks to the generosity of the French Government in giving the benefit of Daguerre's discovery to the world, the process almost immediately made its appearance in England and in America, and during the year 1839, the very year in which Daguerre announced his success, Prof. Morse, then a rising American portrait painter, and Prof. Draper, the well-known chemist, were the first to apply the new process to portraiture. The length of time required for a sitting however, made the newly acquired art, of limited practical value.

In order that this process of Daguerre may be more clearly understood, reference is made to an article by Prof. E. A. Aikin, M.D., of the University of Maryland, published in the Maryland Medical and Surgical Journal, of April, 1840, which is *almost* the *first*, if not the *first* notice of Daguerre's discovery, published in this country. Prof. Aikin divides the process into five parts :

First—Preparing the plate, *i. e.*, plating a thin plate of copper with a thin coating of a silver solution ; then polishing the silver surface with pumice and oil, cleaning it finally with dilute nitric acid, heating, cooling suddenly and repeating the polishing, until the surface became as clear and perfect as a mirror. This part of the process was a very difficult one on account of the liability to lay bare the copper, in which case the work had to be done over again from the beginning.

Second—To apply the coating of iodide. This was

done in a box made for the purpose, in which the polished plate was exposed to the vapor of iodine.

Third—The third operation was the exposure of the plate thus sensitized in the camera box—time of exposure from three to thirty minutes.

Fourth—The fourth operation was the delicate one of development of the image by exposing the plate to the vapor of mercury, which also had to be done in a special box, wherein the mercury was vaporized by means of a spirit lamp; the temperature carefully regulated, and the development of the picture watched through a small glass window.

Fifth—The last operation was the *fixing* in hyposulphite of soda and washing in clean, hot water.

In 1840, a Mr. Goddard of London, and in 1841 M. Claudet of Paris, discovered that the vapor of bromine and chlorine, respectively adding to the iodide of silver a deposit of bromide or chloride of silver, each rendered the plates much more sensitive to the action of light, reducing the time of exposure from thirty minutes to less than one minute. With these discoveries, the daguerreotype seems to have reached its perfection. The period of its utility covered about ten years.

I have called the discovery of Daguerre the birth of photography, but we must not lose sight of the fact that, at the very period when Daguerre and Niepce were pursuing their experimental work, another man, an Englishman, following the lines long before indicated by Wedgwood and Davy, was developing a process for securing the fickle image of the camera obscura, and had actually succeeded when the proclamation of Daguerre's success was made. It is quite probable, or I may say certain, that but for the lucky accident which disclosed the secret of developing the latent image on Daguerre's silvered plates, Fox Talbot would be known to-day as the father of photography.

His process consisted in soaking a sheet of fine writing paper in a weak solution of common salt, and then brushing one side of it with a solution of nitrate of silver. Talcot discovered that paper treated in this way darkened very rapidly in bright sunlight. After an hour's exposure in an ordinary camera obscura, he obtained a distinct impression of the picture formed by the lens. He then *fixed* his prints (*i. e.*, dissolved out the silver salt not acted upon by the light) by washing them thoroughly in water or in a solution of bromide or iodide of potassa. He subsequently adopted the use of iodide of silver instead of the nitrate, but in all other respects his process was very different from that of Daguerre.

He further discovered that, if his sensitized paper were brushed over with a mixture of gallic acid and silver nitrate it was rendered much more sensitive, and the time of exposure correspondingly reduced.

He called his process the Calotype process, and had it patented in England. His pictures were *negatives*, that is, the lights and shades were just the reverse of what they were in nature, the brightest part of a landscape being represented by black patches of reduced silver, while dark shadows, etc., were white. The paper, being semi-transparent, was made more so by the use of wax or some other translucent substance, and positive prints were made therefrom upon sensitized paper.

Fox Talbot, therefore, made the first printing negative, and actually printed the first positives.

To the translucent paper negatives of Talbot we are indebted for another advance, suggested by Sir John Herschel, viz.: the use of glass plates as a support for the sensitive salts of silver. But Sir John's method of preparing such plates by immersing them in a bath of silver chloride until a film of silver had formed upon the surface of the glass, was not successful.

Niepce de St. Victor, a nephew of Daguerre's former

partner, recognized the fact that it was necessary to coat the glass with a film of some suitable substance, in and on which the particles to be affected by the light might rest. After many experiments, he resorted to albumen, mixed with iodide of potassa, bromide of potassa, and common salt, in fixed proportions. The clear liquid was poured upon a glass plate, dried and heated until the albumen hardened and became insoluble. It was then sensitized by being dipped into a bath of silver nitrate; after which it could be at once used, or dried and kept in a dark place until wanted. Such plates were developed in a solution of gallic acid, and *fixed* in the usual way; but their great disadvantage lay in the fact that they required too long exposure to make them of practical value.

For portraiture, the daguerreotype held sway, and for sharp, clear beauty of detail has never been surpassed.

An American (Maynard, of Boston) gave to the world collodion in the year 1847.

The photographers of Europe were quick to seize upon it as a possible coating for plates before sensitizing. There is considerable uncertainty as to who first suggested its use and successfully put the suggestion into practice—Le Gray, a Frenchman, Bingham and Scott Archer, Englishmen, being rival disputants for the honor. I am of the opinion that Le Gray was the first one who actually used a collodion film and obtained upon it a picture, which he did by a process described by him in a treatise published in 1849.

To Scott Archer, however, the photographic world is indebted for the improvement and development of the collodion process, which marked in 1853 the greatest advance since the publication of Daguerre's method, a process known thereafter as the wet-plate process, which in a very short time practically drove daguerreotypes, calotypes, and all the other photo types from the field.

The process invented by Talbot remains substantially

unchanged to the present day, although "innumerable improvements" have been suggested, tried, abandoned and returned in new form to vex the photographer.

As a wet process, it is still used to a limited extent, and we must say of it that it has given us some of the most beautiful photographs which have ever been made, and as a method for making lantern transparencies is still unrivalled. For general purposes, it has always had great disadvantages. Great care has to be exercised in the cleaning and preparation of the plates. They must be used while still in a wet state, and must be developed as quickly as possible after exposure, and before the surface has time to dry.

These drawbacks soon stimulated a great many efforts on both sides of the Atlantic to overcome them, and various methods were employed by some to produce dry plates which would keep until wanted for use; while others devoted themselves to the development of processes for keeping the collodion plates moist. The only successful attempt in either direction which seems to have been made prior to 1866 was made by Dr. Hill Norris, an Englishman, who, by a process which was kept secret, produced dry plates which had good keeping qualities, and were nearly as rapid in action as the wet plates generally used.

Between 1866 and 1879, many improvements in making collodion dry plates were made, but the wet plate process held its own in the studio. Meanwhile, a host of chemists and photographers, both in Europe and America, were striving to utilize some other substance than collodion as the basis of a photographic film, as most if not all the dry plates made had to be sensitized by immersion in a silver bath.

Attention had already been called to gelatine by Le Gray, and most of the experiments were made with that substance. The result was the gelatine bromide dry

plate, which is less than a dozen years old, but which has completely revolutionized the art of photography, and already made possible achievements which the earlier photographers could not have dreamed of. If you ask who discovered or invented it, I point to the long list of the names of those industrious investigators, from Le Gray to Dr. Maddox, who each contributed something; some by failures, and many by successes, toward the grand result. It is safe to say that the gelatine bromide dry plate, almost universally used in making photographic negatives to-day, is the result of no one man's work.

Having said something about other processes, I will now take a moment to explain what a gelatine dry plate of the present day is.

As its name implies, it is a sensitized gelatine film. Gelatine had been suggested as a medium for the retention of the salts of silver upon the surface of glass or paper by Le Gray in 1850, and had been used by many experimenters with varied success. Of these, M. Gaudin, a French photographer, seems to have first recognized the necessity for some sensitive mixture which could always be ready for use by pouring it upon plates, and using them at once, or drying them and preserving them for future use. He was one of those who directed his efforts toward the discovery of such a mixture as early as 1853, and continued his experiments until 1861, when he produced what was known as photogene, a sensitive mixture for coating plates, in which both collodion and gelatine were used in emulsion. The use of plates coated with this emulsion was, however, too uncertain in its results, and Gaudin retired from the field without having realized the accomplishment of his ideal.

One difficulty with most of these early efforts appears to have been that collodion continued to be used in more

or less quantity, and collodion, when dry, hardened into an impenetrable film, which the developing agent could not enter.

About 1870, one Thomas Sutton, in an essay which years after was published in the British journal of photography, gave the first clear, definite suggestion for an improvement of Gaudin's emulsion, in which he proposed that collodion be discarded entirely, and gelatine only be used instead. His theory was that, by using a gelatine emulsion with bromide of silver, a structureless and homogeneous film, exquisitely sensitive withal, would be formed.

He concluded a minute exposition of his theory with these words: "It may turn out that I have done well in digging up this old process of M. Alexis Gaudin, whose name be exalted as the anthor of collodion emulsions and photogenes."

Sutton died soon after, without having seen his theory put to test; but the seed he had sown fell into good ground. Early in 1871, Dr. R. L. Maddox made a gelatine bromide of silver emulsion, with which glass plates and paper were coated, but they were only fairly sensitive, and failed for reasons which are now well recognized. Dr. Maddox was obliged to give up his experiments on account of failing health, but others took up the subject. In 1873, a Mr. Kennett, an amateur, patented a process for making a gelatine bromide of silver coating, which marked a great advance over any of the methods theretofore suggested by Gaudin, Sutton or Maddox. His method was to wash out of the emulsion all of the free salts left therein after the mixture. The omission to perceive the necessity of this was the cause of Maddox's failure. Kennett's plates were really the first satisfactory plates upon the market. In addition to their other good qualities, they were very much more sensitive than any which had previously been used.

Strangely enough, this very fact interfered materially with their general use. Up to this time, photographers had habitually manipulated their plates under an amount of illumination which would prohibit the art at the present day. It took some years and much contention to convince the photographic world that rapid plates, of which Kennett's were the forerunner, could only be manipulated in a chamber from which all of the actinic rays of light were rigorously excluded.

In 1874, M. J. S. Stas, a celebrated Belgian chemist, published in the columns of a Belgian periodical a contribution to the chemistry of photography which gave the clue to the secret of the present system of emulsion making. Stas, however, was not a photographer, and it does not appear that his researches attracted the attention of any who were working in the photographic field. His statement was, and he clearly pointed out, that bromide of silver can exist in at least six well-defined physical states, each having properties peculiar to itself.

These are: 1st. White flakes. 2d. Yellow flakes. 3d. Intense yellow powder. 4th. Pearly white powder. 5th. Yellowish white powder. 6th. Pure intense yellow crystalized state.

He said : "The granular bromide, either dull or shining, and the pearly white modification of it resulting from the action of *boiling water* on the first two, are the most sensitive substances to light with which I am acquainted."

Here was the clue, viz.: the action of *heat.* No one seems to have acted on this suggestion until four years later, when a Mr. Bennett, another amateur photographer, of London, exhibited a number of gelatine negatives before the South London Photographic Society in 1878. One of these was the interior of a room taken by ordinary gaslight after an hour's exposure. Another represented a boat in motion—exposure one-twentieth of

a second ; while others represented exposures between these two extremes.

This exhibition was such a surprising revelation that Mr. Bennett was appealed to to give the secret of his process to the world. This he generously did, and the secret proved that he had profited by Stas' researches, and that the essential point in the process was the application of *heat* to the emulsion. The Liverpool Dry Plate Company soon after put Bennett's plates upon the market.

Of course, Bennett's success stimulated a vast amount of experimental research after some improvement of the process, the most important of which proved to be the boiling of the emulsion itself, by which the sensitiveness was more readily controlled, and the time consumed in the preparation greatly decreased.

Substantially, the process of making dry plates is to-day the process which Bennett gave to the world just eleven years ago. While a great many manufacturers of commercial plates, both in Europe and America, are each claiming some point of superiority, either in sensi tiveness or ease of manipulation, the process of manufacture is substantially as follows :

Certain proportions of pure gelatine, some bromide, either of potassa or ammonium, and distilled water, are combined until the gelatine is well swollen, and then the vessel containing the mixture is placed in a bath of warm water until the ingredients are dissolved ; or, if preferred, the bromide is first dissolved, and then the gelatine added. Nitrate of silver, previously dissolved in water, is then added, together with a small quantity of nitric or acetic acid ; agitating the whole mass very vigorously during these operations. The mass is now placed in boiling water and kept there, with frequent stirring, from one-half of an hour to two hours, according to the sensitiveness required. The mixture is next transferred to a bath of ice water, in which it is allowed to remain until

the mass has assumed a jelly form, when it is squeezed through a coarse canvas, to thoroughly divide up the total bulk into small shreds, which are thoroughly washed, either in running water or in many changes of water, until all of the free nitrate of silver not converted into bromide is washed out. Then the emulsion is carefully remelted, a small quantity of freshly-dissolved gelatine is added, the whole is filtered, and it is ready to be spread upon the paper or glass which is selected to support the negative.

These operations must all be conducted in a room very feebly illuminated by ruby light only, and of course the finished plate must never be exposed to the actinic rays of light until it is exposed in a camera. It is needless to say that the perfection of the dry plate has completely revolutionized photography. It has popularized it, and not only converted it into a valuable adjunct to art, but it has enhanced its commercial value a hundred thousand fold. Moreover, it has opened the door for a most fascinating amusement. A great army of amateurs, already numbering many thousands, and yearly increasing, are tasting the sweets of out-of-door life with an object in view. Men and women, girls and boys, are taken into the fields, the woodland, and along the winding streams, and impelled to observe and study the beauties of nature. This is doing much to build up and sustain an æsthetic side to an age which is inclined to be selfish and prosaic.

For a more extended review of the history of photography, I refer all interested to the very complete work on that subject by W. Jerome Harrison. I wish that time permitted some allusions to the subjects of lenses and printing methods, but these must be reserved for another occasion.

At the conclusion of the address, the subject was discussed by the members present.

APRIL 23, 1889—SEVENTY-FOURTH REGULAR MEETING.

Charles B. Warring, Ph.D., chairman, presiding; twelve members and several guests present.

The chairman, Dr. Warring, read the following paper:

A CURIOUS GYROSCOPIC PHENOMENON—ITS RATIONALE—AN APPARENT LOSS OF ENERGY, AND AN EFFORT TO SHOW WHAT BECOMES OF IT.

If a rapidly revolving gyroscope be held in one's hand, and an attempt be made to turn it quickly from its position to another perpendicular to the first, a strong resistance will be experienced. And this resistance will be proportioned to the abruptness of the change of position. The phenomenon is a very curious one, and never fails to excite surprise.

To study this effect, I devised and had constructed the apparatus before you. (See Fig. 1.)

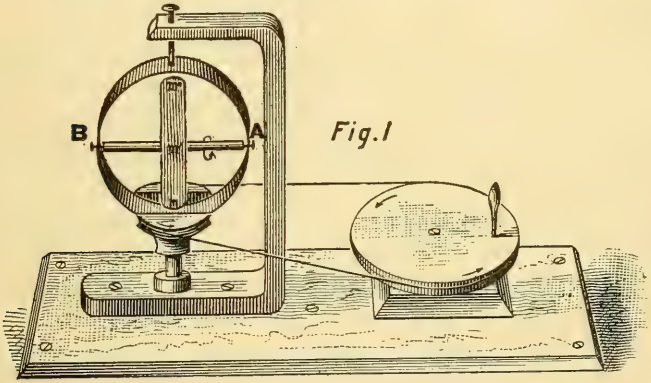


Fig. 1

It consists of an ordinary gyroscope, mounted in a very stiff iron frame, which is securely bolted to the table. The axis of the gyroscope is horizontal, and supported in the usual manner in a ring, which is itself supported by two pins, whose axes and the centre of the wheel, or disc, are in a vertical straight line. The disc revolves

freely in the ring, and the ring revolves very freely on the two pins. Hence, if the disc be set in rapid motion on its axle, and the ring be made to revolve on the pins, we have the gyroscope continuously passing from one plane to another, and we should expect continuous resistance.

On the lower pin is a sort of clutch-wheel, or pulley, which, by help of a lever (not shown in the diagram), can be dropped or raised almost instantly, and so breaks, or makes, connection with the large driving wheel.

First Experiment—I give the disc a high speed in the usual way—by help of a strong string wound on its axle —and time it. I find that friction brings it to rest in, say, five minutes.

Second Experiment—I now let the disc alone. It has ceased to revolve on its horizontal axis. I raise the clutch-wheel as shown in the diagram, and turn the large wheel, and so make the gyroscope revolve very rapidly on the vertical axis. Then I let the clutch drop so as to be out of connection with the ring. The apparatus goes on from its momentum for a very considerable time. The motion is smooth and quiet, until the friction and air bring it to rest. The rotation around the vertical axis did not set the disc to revolve on the horizontal. Apparently it had no effect whatever in that sense. This is what we should expect from the well-known law that forces at right angles to each other neither increase nor diminish the effect of either.

Third Experiment—I now raise the clutch, as in the diagram, and set the disc in very rapid motion on its own axis, and at the same moment attempt to turn the driving wheel, but experience great resistance. While the resistance is still strong—say in fifteen or twenty seconds —I release the clutch-wheel, and almost instantly the horizontal rotation ceases. The disc, however, is still revolving with no apparent decrease of speed. In the pre-

vious experiment—when I did not set the disc to revolving—the horizontal rotation continued for a considerable time after the clutch was released. But now the machine acts as if it had no momentum, or it would be better to say, that it behaves as if the air had suddenly become highly viscid.

Fourth Experiment—We have again the two rotations in planes perpendicular to each other, and both rotations very rapid. In experiment 3, the ring was connected with the driving-wheel only a few seconds, and then released; now the connection is kept up, and I continue to turn the driving-wheel as rapidly as I can against the resistance which the gyroscope offers. This is very decidedly felt, and throws the instrument and the table on which it stands into a violent tremor, rattling whatever happens to be on it. After a little—from twenty to sixty seconds—the resistance seems exhausted, the tremor and rattle cease. No effort is now necessary to keep up the rapid horizontal rotation. I stop the apparatus as quickly as possible, and find that the disc has come to rest. It ought to have run several minutes longer.

These results give rise to three questions:

1. What causes the strong resistance?

2. What causes the disc to come to rest so much sooner in experiment 4 than in experiment 1?

3. What becomes of the large amount of energy expended in overcoming the resistance when the two rotations are going on simultaneously?

As to the first question—the strong resistance. Suppose the instrument in operation, and the movements to be indicated by the arrows in Fig. 1, and very rapid—twenty revolutions or more in a second. Fasten your mental vision upon a molecule, m, at the lowest point in the disc, and in the line of the vertical axis. At that instant it will be absolutely at rest in reference to the horizontal rotation.

In the one-eightieth part of a second it will be carried up 90° to m',* to the horizontal plane passing through the axle of the disc. Its horizontal velocity at m' will be—suppose its radial distance to be two and a half inches—about twenty-five feet a second. This speed is not, at first sight, anything remarkable, but it has been attained in one- eightieth of a second. Gravity—*i. e.*, a body's weight—would impart a speed of only two-fifths of a foot in the same time. Hence the acceleration here is

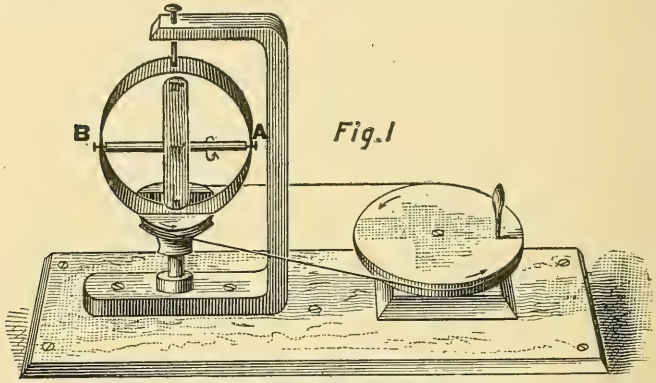


Fig. 1

sixty-two and a half times that of gravity, according to the well-known law, that the accelerating force is proportional to the velocity imparted in equal times.

The effect increases as the product of the number of revolutions in the two planes.

Could we make them sixty and sixty, instead of twenty and twenty, the results would be nine times as great.

It is fortunate that the time this acceleration acts, is so short that the actual velocity attained in our disc is only twenty-five feet in a second, for if it had been permitted to act for a whole second, its speed would be two thousand feet, or nearly twice that of a cannon ball—sufficient to destroy the instrument.

* Unfortunately, the ′ and ″ on the middle and the upper m, Fig. 1, are too indistinct. The reader will please mark them with a pen.

It is no wonder that, when conical bearings (shown in Fig. 2) were used, the pressure upon them forced the ring back so far that the disc was very apt to fly out of its place—in fact, with conical bearings, very little acute (say 60°), and with very high speed, this always occurred. The bearings at first were all of this shape. I found it necessary to change them, and to make them cylinders. After that the disc never flew out.

Fig. 2

In this we have answer to the first question. The resistance comes from the inertia of the successive molecules passing from zero velocity at bot om and top of the disc to maximum velocity as they come to the horizontal plane passing through the horizontal axis.

As to the second question : Why does the disc come to rest more quickly when the ring is in rapid motion ?

It is simply a case of increased friction, produced thus: Fasten your mind on a molecule at m'. It is moving to the right. Its other motion (that in the vertical plane) being at right angles, neither hastens nor retards it, consequently m' endeavors, all the time it is going to the top (m''), to tilt that quadrant toward the right, a tendency which results in a pressure or push upon the pin A. The same thing occurs in the opposite quadrant. In the other two quadrants, as the parts pass from zero velocity to the maximum, there is a downward pull on A and an upward one on B, so that really all four quadrants act in the same sense, and pressure always increases friction. The effect of such increase is seen when I tighten slightly the pins A and B. The time of rotation falls from six minutes to much less than one, and this when the horizontal motion is not going on.

Lastly : What becomes of the additional energy expended to make the ring revolve, when the disc is also revolving ?

Bear in mind that there are two distinct energies; that given to the disc by pulling the string wound around its axle, and that coming through the belt from the driving-wheel. The former is disposed of partly by the friction of the air, and yet more by the friction at A and B, where it passes into heat.

The energy from the driving-wheel may be divided into two parts, the ordinary and the extra-ordinary. The ordinary energy is that needed to overcome the resistance of the air and the friction at the various bearings, and by the belt. This is easily disposed of. It sets up molecular action and changes into heat.

This leaves only the additional energy which must be expended to make the ring revolve when the disc is in motion, and which is not needed the moment the disc comes to rest. A part of this—only a small part—is used in causing an increase of friction on the two vertical pins, for the same tilting tendency that causes pressure at A and B produces a tendency in the ring itself to revolve in its own plane, and hence produces a pressure on one side of each of the two vertical pins. But this absorption of the energy is a matter of accident, as it were. There would be the same increase of horizontal motion from zero to maximum, and the same giving up of motion from maximum to rest as before, for the friction on the bearings has no influence on that. And the amount of energy necessary to increase the movement of even a small fraction of a quadrant—or rather of two quadrants, for the process goes on in two opposite ones at once—from zero to twenty-five feet in one-eightieth of a second, *i. e.*, from 0 to 2,000 feet in a second, is certainly very considerable. If we might imagine energy to be something visible, we should see it taken up continuously in little buckets, as it were, as they pass from m (Fig. 1) to m', and poured out as they pass on from m' to m''. What becomes of the energy poured out? One result is very

evident with my machine. It is put in a violent tremor, and the table, as well as all that is on it, in violent agitation.

I have not yet been able to hold it firm enough to prevent this effect, but I have no doubt that the machine might be bolted so securely as to prevent all visible tremor.

If this can be done, and the rotation of the disc kept up by an electric magnet, I can see but one escape for the energy, that is in some form of molecular action, probably heat. I will hazard the conjecture that a disc compelled to perform both rotations simultaneously will, after a time, show an increase of temperature.

At the conclusion of the paper, the chairman made the regular annual report of the work of the Section during the year and the Section elected the following:

Chairman, . . .	Mr. Edward Elsworth.
Secretary, ...	Mr. James Winne.
Curator, ...	W. G. Stevenson, M. D.
Librarian, . . .	Mr. Fred. S. Arnold.

NOVEMBER 19, 1889—SEVENTY-FIFTH REGULAR MEETING.

Edward Elsworth, chairman, presiding; eleven members and several guests present.

The chairman of the section, Mr. Edward Elsworth, presented the subject of Food Adulterations, and spoke of the importance of the subject to society. He criticised the extravagance of the Legislature and of local boards in matters of comparatively lesser interest and compared it with the small appropriation made by the State of New York in support of the State Board of Health.

One of the purposes of this society should be to aid local boards in establishing sanitary appliances, securing a good and healthful water supply, including proper sys-

tems of sewage, and the prevention of the sale of adulterated articles of food.

The excuses for adulteration are competition in trade, and a demand for cheap goods, hence we find that the principal articles of food which are adulterated are those which enter most largely into domestic consumption, viz: tea, coffee, milk, canned goods, sugar, wine, beer, flour, confectionery, butter, cheese, and spices.

To illustrate the extent to which such adulteration is carried, the record of examinations made in the city of New York in the year 1882, 52% of the butter examined showed adulteration; 56% of olive oil, 62% of spices and 90% of coffee; while of flour, only 6% exhibited any traces of adulteration.

This somewhat alarming exhibit is modified by the fact that most of the adulterations are not positively injurious, but are produced by the admixture of some cheaper substance, to increase weight or bulk. For instance, olive oil is adulterated by the addition of cotton seed oil—butter by oleomargarine—cheese by lard or cottonseed oil—coffee by chicory, peas, rye, corn and some coloring matter.

The danger in using canned goods, lies in the possibility of metallic poisoning, resulting from careless and imperfect methods of canning.

A very considerable part of the time occupied by the chairman was devoted to a description of the growth and curing of tea, aided by illustrations upon the blackboard.

The structure and shape of the genuine tea leaf were explained, and the various methods of adulteration and "fixing" by the addition of dangerous substances to add strength and impart color and brilliancy were fully exposed.

Dr. Warring nominated for membership in the Scientific Section, Mr. J. B. T. Tuthill.

DECEMBER 10, 1890—SEVENTY-SIXTH REGULAR MEETING.

Edward Elsworth, chairman, presiding.

Mr. Gilbert Van Ingen delivered a very interesting address on the subject of "Ferns," and submitted to the section the following list:

Polypodium vulgare, *L.*—Woods; growing on stony soil and over rocks: common.

This species varies greatly. The most common form is that with the leaves broadest below the middle; pinnæ with few serrations and with obtuse to slightly acute tips; and sharp sinuses. A second form with frond of an elongate-triangular outline and with elongate-acute pinnæ is sometimes found on wet shaded rocks. A third and rarer form is one in which the leaf is of a lanceolate outline; 10 to 12 inches long; two inches wide at middle from which point it tapers gently toward both ends; and with pinnæ and sinuses rounded.

Adiantum pedatum, *L.*—Rich stony soil: common in all the woods.

This species has only one constant variety. In this variety the leaf forks at the summit of the petiole and remains simple.

Pteris aquilina, *L.*—Very common in all open woods.

Cheilanthes vestita, *Swz.*—Shaded rocks: not rare.

This elsewhere rare fern is here rather common, growing on all the cliffs along the shores of the Hudson River and in some places forming patches two and three feet in diameter with leaves six and eight inches long.

Pellæa atropurpurea, *Link.*

Common on the limestone cliffs east of the city. It is occasionally found on the slate cliffs and then is much stunted in growth.

Woodwardia Virginica, *Smith.*—Swamps: frequent.

It has been rarely found in fruit.

Asplenium ebenoides, *Scott.*—Limestone cliffs: very rare.

Found on limestone cliffs southeast of Poughkeepsie in 1882–3.

Asplenium trichomanes, *L.*—Common on rocks and on rocky soil.

Asplenium viride, *Huds.*—Summit of Shawangunk Mts: rare.

Asplenium ebeneum, *Ait.*—Common on rocky soil and on cliffs.

Asplenium montanum, *Willd.*—Quite abundant on the cliffs of the Shawangunk Mts.

Asplenium ruta-muraria, *L.*—Rare: the only station being "Cedar Ridge" an outcrop of Calciferous sandstone three miles east of Poughkeepsie.

Asplenium thelypteroides, *Mx.*—Swamps: uncommon. Rather abundant in "The Glen" at Vassar College.

Asplenium filix-foemina, *Bernh.*—Common in all the woods.

Asplenium Bradleyi, *Eaton.*—Summits of the Shawangunk Mts.: rare.

Camptosorus rhizophyllus, *Link.*—Not uncommon on outcrops of slate.

There have occasionally been found leaves with the auricles at the base greatly elongated. These elongated auricles have been observed to take root at their apices in the same manner as the midrib of the leaf.

Phegopteris hexagonoptera, *Feé.*—Common in moist woods.

Phegopteris polypodioides, *Feé.*—Swampy hillsides: rare.

It has been found with *P. hexagonoptera* along the small stream that flows under "Table Rock" cliff back of Chestnut Grove opposite Poughkeepsie.

Phegopteris dryopteris, *Feé.*—

There was formerly a patch of this fern in an orchard back of Lewisburg, opposite Poughkeepsie. It was destroyed by plowing a few years ago.

Aspidium thelypteris, *Swz.*—Marshes—swampy woods : very common.

Aspidium Noveboracense, *Swz.*—Moist woods and swamps : common.

Aspidium spinulosum, *Swz.*—Frequent in rich woods.

Aspidium spinulosum, *var intermedium*, *Eaton.*—Common in damp woods.

Aspidium cristatum, *Swz.*—Common in swampy woods.

Aspidium cristatum, *var Clintonianum*, *Eaton.*—Frequent in swamps.

Aspidium marginale, *Swz.*—Common in dry stony soil.

Aspidium acrostichoides, *Swz.*—Common in dry woods.

Aspidium acrostichoides, *var incisum*, *Gray.*—Frequent.

Cystopteris bulbifera, *Bernh.*—In alluvial soil along streams : common.

Cystopteris fragilis, *Bernh.*—Shaded rocks : common.

Woodsia obtusa, *Torrey.*—Common on cliffs along the Hudson River.

Woodsia ilvensis, *R. Br.*—Common on exposed cliffs. Occasionally found in damp shady places with nearly glabrous leaves.

Dicksonia pilosiuscula, *Willd.*–Along streams : common.

Osmunda regalis, *L.*—Swamps : common.

An inconstant form is occasionally found in which the upper portion of the fertile part of the leaf is sterile.

Osmunda Claytoniana, *L.*—Wet low ground : common.

Osmunda cinnamomea, *L.*—Wet low ground : common. Variety *frondosa*, *Gray*, is occasionally found.

Onoclea struthiopteris, *Hoffm.*—Common in the alluvial soil along Wappingers Creek, where it fruits abundantly.

Onoclea sensibilis, *L.*—Fields : common. Variety *obtusilobata*, *Torr.* is frequent.

Ophioglossum vulgatum, *L.*—Peat bogs : rare. Discovered September, 1888, in a peat bog near a small

house on the Smiley farm back of the Hudson River Driving Park. The next nearest locality is Pine Plains, Dutchess Co., N. Y.

Botrychium ternatum, Swz.—Rare on dry shaded ground.

Botrychium ternatum, var obliquum, Muhl.—Common in dry fields.

Botrychium ternatum, var dissectum, Gray.—Common in dry fields.

Botrychium Virginianum, Swz.—Common in damp woods.

The total number of species and varieties listed is 43. Of this number, one *Aspidium acrostichoides incisum* is not constant. One other *Phegopteris dryopteris* has had its locality destroyed. Three, *Asplenium montanum*, *A. Bradleyi* and *A. viridis* are found only on the Shawangunk Mts., 15 miles west of Poughkeepsie. The remaining 39 species and varieties are all found within a radius of three miles of Poughkeepsie.

JANUARY 10, 1890—SEVENTY-SEVENTH REGULAR MEETING.

Edward Elsworth, chairman, presiding; seven members and thirty guests present.

W. G. Stevenson, M.D., read a paper on "The Imagination."

JANUARY 28, 1890.—SEVENTY-EIGHTH REGULAR MEETING.

Edward Elsworth, chairman, presiding; eleven members and about thirty guests present.

The following paper was read by Charles B. Warring, Ph. D.:

THE THEORY OF THE BICYCLE.

We have all seen rope-walkers. Probably most of us have tried to walk on the top-rail of a fence, and have a vivid recollection of the incessant tossing of arms and legs to keep our balance, and how great assistance we got from a long stick, or even a stone held in our hands.

But the bicyclist gets no help from stick or stone. His legs move only in the tread of the wheel, and his hands rest quietly on the ends of the cross-bar of his machine. Apparently there is entire repose, while the rope-walker keeps every muscle tense, and every limb in motion, or ready to move.

No wonder when a tourist on his 'cycle spins for the first time, through a village here, or among the nomads of Asia, he is followed by wondering beholders till his machine carries him out of their sight.

We involuntarily ask, how is it possible for one supported on so narrow a base to keep his seat so securely, and apparently without effort?

For an answer to this question, I have searched somewhat widely, and while I have found articles enough on, or about, the bicycle, I have found none that offers a reasonable explanation. This is my apology for presenting the present paper, in which I shall state the theories which have been offered; the reasons why I consider them unsatisfactory, and then, give what I regard as the true rationale of the machine.

The only formal paper I found, that claimed to explain the bicycle, was one by Mr. C. Vernon Boys, entitled "The Bicycle and its Theory." It was delivered before a meeting of Mechanical Engineers, and is reported at great length in *Nature*, Vol. 29, page 478. Here, thought I, is probably something valuable and convincing. But,

on examination, I found that, of the several pages of closely-printed matter, the "theory" occupied possibly a dozen lines, all the rest was about the bicycle, but not about its theory.

In those dozen lines we are told that Mr. Boys exhibited a top in action, and requested his audience to notice its remarkable stability. Then he said that the stability of the bicycle was due to the same principles, but made no attempt to show any connection between them.

The top revolves on its axis, and it stays up, as you see; the wheel of the bicycle revolves on its axis, therefore it stays up, was his theory and demonstration, and the whole of it, and so far as we can judge from the report, he was satisfied, however it may have been with his audience.

Of all machines, probably none is so little understood as the top, and its near relative, the gyroscope. Hence the best that can be said is, that the lecturer merely availed himself of that tendency, found in most minds, to explain an unfamiliar phenomenon by referring it to some other more familiar phenomenon, longer known, but equally incomprehensible. As if, as in grammar two negatives make an affirmative, so, in physics, two unknowns make a known.

Without going into the theory of the top,—the gyroscope, it is easy to show that their stability, and that of the bicycle must be due to different principles. I spin before you on the table a top with a somewhat blunt point. You notice that it runs around in a circular, or spiral path, and gradually rises till it is perpendicular to the table. I strike it quite a hard blow, but do not upset it. In fact it is hardly possible to strike it with force enough to upset it. I send it flying across the table,

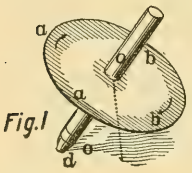


Fig. I

or off to the floor, but still it maintains its upright position.

You notice that it not only runs about in a spiral, but that it swings, or gyrates, around a vertical axis. If the point, instead of being somewhat blunt, is very fine and sharp, and well centred, the top will scarcely travel at all, but will swing around an almost stationary vertical axis. If the point—very fine and sharp, and well centered, remember—happens to fall into a slight pit in the surface of the table, it will cease to travel, but will continue for a very considerable time to gyrate around a vertical, and will be remarkably stable, no matter at what angle it leans. Hence, it follows that it is not the travelling of the top which keeps it up. You may stop that entirely with no sensible effect upon its staying up, but the instant—even in the case of one that leans ever so little—I prevent the rotation around a vertical, it falls. I vary the experiment in every possible way, the result is the same; the moment the swinging—or, gyration, as it is called—around a vertical ceases, the top falls.

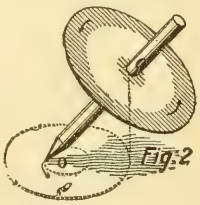


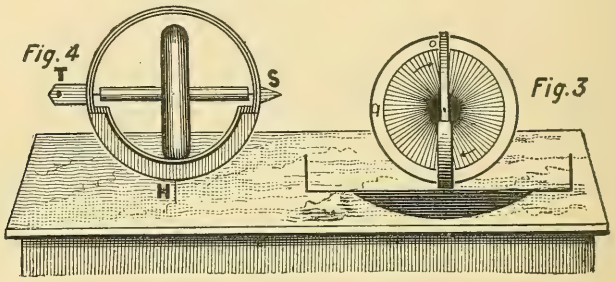
Fig. 2

Certainly, in case of the bicycle, there is no swinging—gyrating—around a vertical axis. Whatever else the machine may do, it does not do that.

We may, I think, dismiss the top from further consideration; but there is another instrument apparently much closer in its relationship to the bicycle. I have here a gyroscope with its wheel upright like that of a bicycle. (See Figs. 3 and 4.) The lower part of the ring rests in a kind of trough, to the bottom of which is attached crosswise a piece of metal (best seen in Fig. 3) curved on the lower edge, and with two projecting wires by which it may be drawn back and forth in the plane of the wheel.

I will now set the wheel in rapid motion, much more rapid than any bicycle wheel can go. I place the instrument—its name in this particular form is gyrostat—I place it on a smooth, hard surface,—I have here a pane of glass,—and leave it to itself.

At once it begins, as you see, to revolve around a vertical axis. If it leans little, it revolves slowly; if it leans much, it revolves faster.



It retains its upright position, though I push it, or even strike it a violent blow. It resists with remarkable force. It fact it is scarcely possible to upset it even by the impact of a hammer, providing the blow be delivered on either S or T, and in a vertical plane. You may hang a weight of a pound or more on T or S, and although standing on only a knife-edge, the instrument will not fall over. The only effect, as you see, is to make it spin more rapidly around a line drawn perpendicular to the table at the point of contact.

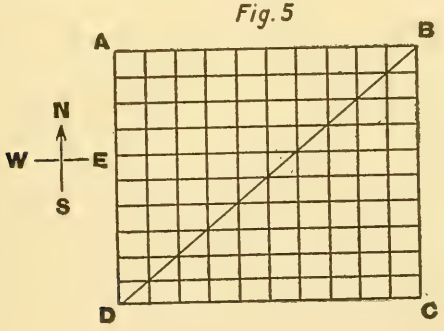
I now take it by the projecting wires, and attempt to make it move in a straight course, as a bicycle does when it spins along the road. Instantly it falls. The rotation of the wheel on its axis was not, in the slightest degree, interfered with; but the stability vanished the moment the rotation around the verticle axis ceased. Yet, you observe, the conditions are far more favorable to stability than in the bicycle, *if* the latter's stability is due to gy-

rostatic action, for the mass of the rim of our gyrostat is many times heavier in proportion, and its speed incomparably greater. I try it over and over, the result is always the same. No amount of skillful management will make the instrument stay erect for an instant if it must move in a straight line. I submit that this and the previous experiments are proof positive that the sustaining power of a bicycle *does not* come from the "gyrostatic action of its wheel."

Others find the reason why the Bicycle does not fall, in "its going so fast," referring of course, in a blind way to that principle embodied by Newton in his first law; "A body in motion, if left to itself, will continue in motion in a straight line" for ever. It is true the bicycle goes very fast, but does that avail anything in solving the problem of its stability? Let us see.

It is another principle in physics that two forces acting at right angles to each other, do not interfere. Each produces its own effect as fully as if the other did not exist. For example, if a certain force sends a body, D,

Fig. 5



north at the rate of 24 feet in a second, and another force sends it east 24 feet in the same time, at the end of one second it will have gone 24 feet north, and 24 feet east, exactly as if each force had acted alone. Going towards A B, does not in the least hinder its going towards B

C. Now in case of a bicyclist, his forward motion is at right angles to gravity, hence does not in any way resist it, and therefore, as it is gravity that causes him to tilt over, the forward motion will not prevent his falling.

But it may be said that the force of quality is in fact resolved into two components, one vertical, and the other horizontal, and that it is only the latter that causes the 'cyclist to fall. This does not affect our conclusion, for both components are perpendicular to the course of the bicycle, and hence its forward motion can in no way counteract either of them.

Unless, therefore, some other force beside his forward movement comes into action, the 'cyclist must fall towards which ever side he happens to begin to lean. Many think they find this in "Centrifugal Force." You are all familiar with the effects of this so called force. You feel them every time you turn a corner quickly, whether on foot, in a wagon or on horse back. The bare-back riders in the circus lean well towards the centre of the ring to escape being thrown outward.

We see its effect when the 'cyclist spins around a corner. In this case "centrifugal force" plays an important part and is the real upholding force. But centrifugal force is impossible, so long as the body moves in one direction. Other things being equal, it is greater in proportion to the abruptness of the change, or as mathematicians say, the centrifugal force, so long as the velocity is uniform, varies inversely as the radius of the curve in which the body moves. If that is very large, the centrifugal force will be very small. If the radius of curvature becomes infinite, *i. e.*, if the curve becomes a straight line, the centrifugal force becomes infinitely small, or zero. So long, therefore, as the bicyclist does not turn any corners—keeps in a straight course—the "centrifugal force" gives no assistance, whatever, in

understanding the secret of his keeping his seat so securely.

But although this is true, this force may be thought sufficient, if supplemented by skillful balancing. The centrifugal force keeps the machine from falling when turning corners; Will not good balancing account for its stability when moving in a straight line? We are all familiar with the phenomena of keeping one's balance. We know the help a heavy pole gives at such times; how one's arms and legs move with startling rapidity in the direction opposite to that in which he feels himself falling. Certainly there is none of this on the wheel. If the stability was due to balancing, it would not be difficult for the bicyclist to sit on his machine when not in motion, and with its wheels pointing both in the same direction. I have never seen one that could do it. I suspect, however, that it is not any more impossible than to stand on the top round of an unsupported ladder. But the ordinary 'cyclist cannot do it, and yet, without apparent effort, he rides securely. That his stability is not due to his balancing and to the rapid forward motion combined, is evident when we reflect that if the handles are immovable, so that neither of the wheels can be turned to the right or left, it is impossible for any ordinary man to keep his machine from falling, unless he can change its course from a straight line to a curve, no matter at what speed he may move.*

I think I am justified in saying that, of all the reasons thus far assigned for the stability of the bicycle, none is

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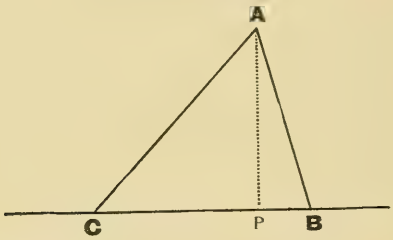
*At the close of the reading of this paper, a teacher of the art of riding the 'cycle, a man of large and varied experience, arose, and in the course of his remarks said that one of the chief difficulties he had to contend with in teaching beginners to ride was to induce them to give up all idea of balancing; that till this was done they could not ride well—a striking corroboration of theoretical conclusions arrived at by the writer of the paper.

satisfactory. Gyration has absolutely nothing to do with it. Centrifugal force has no application, save in turning corners, or otherwise changing abruptly the direction of the movement. Balancing is of no use. Rapid motion accounts for nothing.

Some other explanation is needed. This I shall now attempt to give.

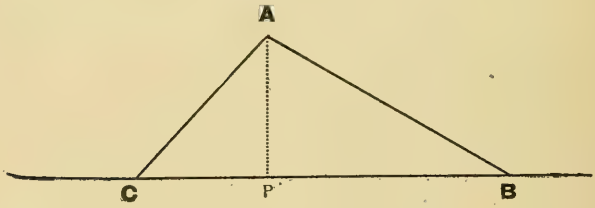
It will aid us if we omit from an ideal bicycle all that is not essential to our purpose. I will therefore draw on the blackboard two skeleton diagrams of the bicycle, representing the lines of force in the ordinary kind,

Fig. 6.



and in the "Safety,"

Fig. 7

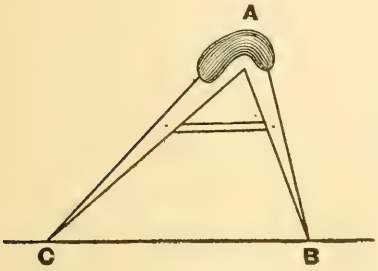


A is a point in the centre of the saddle; *B* is the point where the fore wheel touches the ground, and *C* where the other. *A B* and *A C*, therefore, represent the lines of force when the instrument is upright and at rest. The weight of the rider brings the centre of gravity of the whole instrument very near the saddle.

It is evident that *A* cannot fall backward or forward, since a vertical from it falls between the points *C* and *B*. In reference to these, *A* is in, what physicists call stable equilibrim, while in regard to side motion its equilibrim is very unstable. The least thing will upset it.

For experimental purposes, I have had a skeleton made the same in form as the last diagram, but strong enough to be safely handled.

Fig. 8



The shorter leg is 5 feet long; the other about 7 feet. The lead saddle weighs 6 pounds.

It consists as you see of two long, slender pieces of pine, and looks like a huge capital *A*, the cross piece serving merely to hold the whole more firmly together. At the apex, or saddle *A*, I have placed a few pounds of lead to represent the rider.

In the ordinary bicycles there is a large wheel in front, and a very small one behind, and the saddle well over the former. In these the forward leg *A B*, fig 6, is much the steeper and shorter. In "Safety" 'cycles it is just the other way. At present we will consider only the former. For convenience in handling, and that it may be better seen, I place the foot C, the rear one of my skeleton frame, on the table, and hold the other, B, in my hand, and at the same height from the floor.

Now notice. The weight tilts toward the east, I quickly move the lower end toward the east till it comes under the weight. If it tilts toward the west, I move my hand

quickly towards the west. In every case by moving my hand more rapidly than the weight tilts, I bring the point of support under it. It is very easy in this way to keep it from falling.

But how can the rider move the point of support when it is on the ground, several feet out of his reach?

He does it by turning the wheel to the right, or left, as may be necessary, that is by pulling the cross-bar to the right or left, and thus turning the forked spindle between whose arms the forward wheel is held and guided.

But some one will ask, How does turning the wheel bring the point of support to the right or to the left—which ever way the machine may be tilting?

Let us suppose a 'cyclist mounted on his wheel and riding, say, towards the north. He finds himself beginning to tilt towards the right. He is not only going north with the machine, but he is going east. He turns the wheel eastward, the point of support at once begins to go eastwardly, and as it goes much faster than the machine tilts—at first—it quickly gets under him, and the wheel is again upright. To one standing at a disttance, in front or rear, the bottom of the wheel will be seen to move to the right or left, just as a few moments ago, I moved the foot of the frame which I exhibited.

Here then we have the explanation sought. The stability of the Bicycle is due to changing the direction of the wheel to the right or left, which ever way the leaning is, and thus keeping the point where it rests on the ground, under the rider.

It may be questioned whether the bottom point of the wheel really travels faster than the weight at the saddle tilts over, and, if it does not travel faster, the explanation which I have been giving fails. This can readily be answered. By an easy calculation based on the principle well known in physics, that the velocity of a body moving

under the influence of gravity varies as the square root of the height from which it has fallen, irrespective of the character of the path it has described, I find that in case the rider's seat is, for example, 60 inches from the ground, and the machine has inclined 6 inches from the perpendicular, it is at that instant, if free to fall, moving laterally at the rate of only $1\frac{1}{4}$ feet a second, or 4,500 feet an hour—much less than a mile. But six inches is a large amount, a good rider does not tilt that much; we will suppose him out only 3 inches, then his lateral movement will be at the rate of only some 2,200 feet in an hour. If the tilt is less, the falling rate will be less. To keep the centre of gravity over the base, the bottom of the wheel needs only to move to the right or left—which ever way the machine is leaning—somewhat faster than these slow rates.

There is no great difficulty in doing this, for if the bicycle is going 8 miles an hour it is necessary to change its course only about 7 degrees; if 4 miles, then only about 14 degrees; if 2 miles, then about 28 degrees. The angle grows smaller as the speed grows greater; at 16 miles an hour, the wheel would need to be turned thro an angle of less than two degrees. From which follows the fact well known to 'cyclists, that the slower the machine is traveling the more the handles must be turned, and the more difficult to keep from falling.

From the fact that the bicycle keeps up by keeping its supporting point under it, like a pole standing vertically on one's finger, some curious, and to most people, quite unexpected results follow. The higher the machine, the less the danger of falling sideways; and, as the load is in the saddle, it is the height of the saddle and not the size of the wheel, that affects the lateral stability. Everybody knows that it is easier to support a long pole than a short one. I have here three rods, respectively one foot, three feet, and seven feet long. I balance the

last very easily, the second, not so easily, and the first, only with considerable difficulty.

The heavier the load on a bicycle, other things being equal, the more easy to keep it from falling to either side. Therefore when a rider carries a load on his shoulders, the weight really helps him to keep his upright position.

I have a cap of lead weighing four or five pounds, which I slip on the top of each of these rods, in succession. In every case, the stability is increased, but most so upon the longest rod.

Experts in their exhibitions, sometimes put both legs over the handle-bar, and ride with safety. But there is nothing remarkable in this, for their legs, placed on the bar, raise the centre of gravity, and hence add to the stability. It is only necessary that, in some way they should be able to turn the bar, and they can ride until the momentum is exhausted.

A much more difficult feat is to ride on one wheel. The small wheel—the rider holding the other in the air—is most easily managed. It is merely a case of supporting on a small base some long, upright body. One keeps moving the point of support so as to keep it under the centre of gravity. It needs only a quick eye and a steady hand. It is much more difficult when the 'cyclist uses only the big wheel, the other having been removed, for he is liable to fall forward, or backward, or to either side. To avoid the first and second, he leans forward a little beyond his base, and would pitch headlong, but that he drives the wheel forward by the treadles just fast enough to prevent it. We all do the same thing when we walk. We lean so far forward that we would fall, did we not keep moving our feet fast enough to prevent it. On the single wheel, most of us would fail, because, from lack of experience, we would make the wheel go too fast, and so would fall backward; or else, not fast

enough to keep from falling on our faces. As to falling sideways, that is prevented exactly as when both wheels are used, the 'cyclist turns the cross-bar to right or left. Experience, a level head, and a steady hand tell him how far to turn it.

From what has been shown, we may conclude that the danger of a header increases as the height of the saddle grows greater; and decreases as the saddle is put further back.

The danger of tilting over sidewise grows less as the height of the saddle is increased.

But besides this, another element of the problem comes into action. I refer to the relative ease of managing the different kinds of 'cycles.

Ability to keep from falling to either side, depends upon the rider's ability to keep B (the front wheel) Fig. 6, or 7, in the vertical plane passing through C and A; or, in other words, to make B move promptly to right or left as the machine happens to lean. If A were directly over B, then if A leaned three inches it would be necessary to move B only three inches. If A be moved back until it is half way between C and B, (C, so far as lateral movement is concerned, is always at rest) then to bring B into the vertical plane passing through C and A, it must be moved six inches to the leaning side. If from C to B is four feet, and A is only one foot forward of C, then B must move twelve inches to correct the same inclination as before. If A is moved back until it is just over C, then the number of inches necessary to bring B into the vertical plane, would be infinite, which is only the mathematical way of saying that it would then be impossible by moving B, to keep the machine from falling over.

Calling the distance $C\,P$, a, and $P\,B$, b, we have the lateral stability $= \frac{a}{a+b}$. If we let b be a constant quan-

tity, and a, a variable one, we can increase the lateral stability by increasing a, or in other words by putting C further back, leaving the rest of the machine as it is. If $a=1$ foot and $b=2\frac{1}{2}$ feet, then lateral stability $=\frac{1}{1+2\frac{1}{2}}=\frac{2}{7}$. If we make $a=2\frac{1}{2}$ feet, then lateral stability$=\frac{2\frac{1}{2}}{2\frac{1}{2}+2\frac{1}{2}}=\frac{1}{2}$ consequently the two stabilities are to each other as 1 to $1\frac{3}{4}$, which means, being interpreted, that of two bicycles of equal height one measuring from center of wheel to center of wheel $3\frac{1}{2}$ feet and the other 5 feet, and the saddle in each $2\frac{1}{2}$ feet behind center of fore wheel, the lateral stability of the latter will be $1\frac{3}{4}$ times that of the other. If from seat to rear wheel center be 5 feet, and to front center $2\frac{1}{2}$, then the stability will be 2.3 times greater than the first. The gain would hardly be worth the increase of expense and awkwardness in so long a machine.

FEBRUARY 18, 1890—SEVENTY-NINTH REGULAR MEETING.

Edward Elsworth, chairman, presiding; ten members and thirty guests present.

The following question was reviewed historically, and the present view of science presented: "Elecricity. What is It?" by Le Roy C. Cooley, Ph.D.

MARCH 1, 1890—EIGHTIETH REGULAR MEETING.

Edward Elsworth, chairman, presiding.

In the absence of the secretary, Mr. Fred. S. Arnold was chosen secretary *pro tem.*

The meeting was of a social and informal character, no paper being presented.

W. G. Stevenson, M.D., nominated Mr. Silas Wodell for membership of the section, and Prof. L. C. Cooley nominated Dr. Theodor Neumann.

MARCH 25, 1890—EIGHTY-FIRST REGULAR MEETING.

Edward Elsworth, chairman, presiding; eight members and ten guests present.

In the absence of the secretary, Mr. Fred. S. Arnold was chosen secretary *pro tem.*

The curator reported the gift of a collection of land shells by Miss Anna Goodsell.

Mr. Gilbert Van Ingen gave a comprehensive address on the subject of "The Land Snails of Poughkeepsie," and submitted to the Section the following:

A PRELIMINARY LIST OF THE LAND SNAILS OF POUGHKEEPSIE, N. Y.

The following list contains the names of all species of land snails which have been detected in the immediate vicinity of Poughkeepsie, N. Y.

It is hoped that in the near future we will be able to present a paper on the distribution of snails in Dutchess and adjacent counties.

GEOPHILA ELASMOGNATHA.

Fam: Limacidæ.

Gen: Limax.

Limax maximus, *Linn.*—Common in cellars and occasionally in hot houses.

Limax flavus, *Linn.*—Hothouses; common.

Limax agrestis, *Müll.*—Hothouses; infrequent.

Limax campestris, *Binn.*—Fields and gardens; common.

Fam. Zonitidæ.

Gen: Zonites.

Zonites fuliginosus, *Griff.*—In rich woods under leaf mould; not common. Glen at Vassar College; hills about Highland, Ulster County.

Zonites cellarius, *Müll.*—Very abundant in hothouses.

Zonites alliarius, *Müll.*—Hothouses ; frequent.

This species is interesting on account of the strong garlic odor disseminated by the animal when it is disturbed.

Zonites nitidus, *Müll.*—Under logs, low ground ; common.

Zonites arboreus, *Say.*—Under logs, loose bark and stones in moist places ; common.

Zonites radiatulus, *Alder.*—Under logs on low ground ; common.

Zonites indentatus, *Say.*—In stony soil ; common.

Zonites minusculus, *Binn.*—In stony soil ; not common. Bech's woods; James' woods; Glen at Vassar College.

Zonites fulvus, *Drap.*—In leaf mould ; infrequent. James' woods; Glen at Vassar College; Bech's woods.

Zonites internus, *Say.*

Three dead mature shells and one live young shell of this species were found in April, 1890, under leaves on hillside above the ferry landing at Highland, Ulster Co.

Zonites multidentatus, *Binn.*—Stony soil and leaf mould on hillsides—Cliffs at Sunfish Cove ; Bech's Woods.

Family Philomycidæ.

Gen. Tebennophorus.

Tebennophorus carolinensis, *Bosc.*—Dead logs and leaf mould ; rare. James' Woods ; hills back of Highland Station, Ulster Co.

Fam. Helicidæ.

Gen. Arion.

Arion Fuscus, *Müll.*—A few specimens found in a hothouse.

Gen. Patula.

Patula alternata, *Say.*—Under bark, rotten logs and stones : common.

Patula striatella, *Anthony*.– Under bark, rotten logs and leaf mould in cold, wet places: not common. Glen at Vassar College.

Patula Asteriscus, *Morse*.—In leaf mould in cold swamps: rare.

Three specimens have been found in "The Glen" at Vassar College.

Gen. Helicodiscus.

Helicodiscus lineatus, *Say*.—Stony soil: common.

Gen. Punctum.

Punctum pygmaeum minutissimum, *Lea*.—Stony soil: not common. James' Woods; Glen at Vassar College; Bech's Woods.

Gen. Mesodon.

Mesodon thyroides, *Say*.—Rich, shady woods: common.

Very abundant about Winslow's marl pond, three-quarters of a mile south of the Asylum.

M. thyroides bucculenta, *Gld*.—Leaf mould: not common. Glen at Vassar College.

M. albolabris, *Say*.—Hillsides: common. Prefers stony soil.

Gen. Stenotrema.

Stenotrema hirsutum, *Say*.—Under stones and in leaf mould: common.

Stenotrema monodon, *Rack*.—Under logs and stones: not common.

Stenotrema monodon, *fraterna Say*.—Under logs and stones: not common.

Gen. Triodopsis.

Triodopsis tridentata, *Say*.—Under stones in rich woods: common.

Gen. Vallonia.

Vallonia pulchella, *Müll*.—Under stones, logs and boards; in places very abundant.

Gen. Strobila.

Strobila labyrinthica, Say.—In leaf mould and stony soil; not common; Bech's woods, James' woods, Glen at Vassar College.

Fam. Pupidæ.

Gen. Pupa.

Pupa fallax, Say.—Common on the cliffs along the Hudson River.

Pupa corticaria, Say.—On bark and in moss and grass on cliffs; common.

Pupa rupicola, Say.—In tufts of grass on cliffs; rare. Cedar Ridge; cliffs at Sunfish Cove.

Pupa armifera, Say.—In grass on cliffs and on rotten logs; common.

Pupa contracta, Say.—Under loose bark and in grass tufts on cliffs; common.

Pupa edentula simplex, Gould.—In leaf mould in cold swamps; rare. Glen at Vassar College.

Gen. Vertigo.

Vertigo ovata, Say.—Under loose bark and logs; not common. Low ground near the Hudson River Driving Park.

Vertigo bollesiana, Morse.—Leaf mould and loose bark; not common. Glen at Vassar College.

Fam. Stenogyridæ.

Gen. Ferussacia.

Ferussacia subcylindrica, Linn.—Under leaf mould in low ground; not common. Glen at Vassar College; Bech's woods.

Family Succinidæ.

Gen. Succinea.

Succinea avara, Say.—In grass on cliffs and hillsides; also in cold swamps; common. Cliffs along Hudson River; Glen at Vassar College.

Succinea obliqua, *Say.*—Cold swamps and marshes; common. Bech's woods; Glen at Vassar College.

Succinea ovalis, *Gould.*—Marshy places; common. Bech's woods; marshes along Hudson River.

APRIL 8, 1890.—EIGHTY-SECOND REGULAR MEETING.

Edward Elsworth, chairman, presiding; eight members and thirteen guests present.

The following address was given, accompanied by experiments in demonstration:

SIR WILLIAM THOMSON AND HIS "GYROSTATIC BALANCE," AND THE RELATION OF NUTATION TO PRECESSION.

BY CHARLES B. WARRING, Ph.D.

In an address before the Mathematical and Physical Section of the British Association, at Montreal, Sir William Thomson described what he called a Gyrostatic Spring Balance, illustrating his description by a drawing of the instrument. His object was to aid in "steps towards a kinetic theory of matter."

He says "it is scarcely possible to help anticipating, in idea, the arrival at a complete theory of matter in which all its properties will be seen to be merely attributes of motion." He adds, "The kinetic theory of gases stops absolutely short at the atom or molecule, and gives not even a suggestion towards explaining the properties in virtue of which the atoms, or molecules, mutually influence one another."

After pointing out what he styles a "fatal fault" in the kinetic theory of gases, he says, "Even if this fatal fault did not exist, there would exist beyond it a grander theory,—to explain the elasticity of solids."

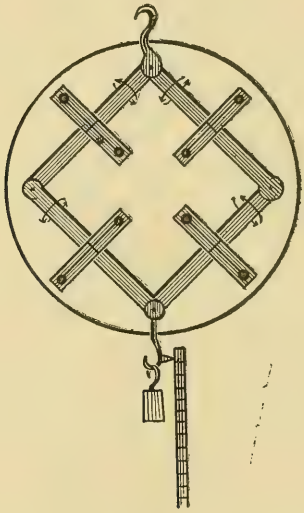
And it was to aid in this that he devised the Gyrostatic Balance. He then describes the instrument by help of

the "Scientific imagination," and tells what it will, and what it will not do.

To the kinetic theory of gases, I had given but little attention, and still less to that of solids, but I had given a good deal to the laws of gyrating bodies. When I read that part of Sir William Thomson's address, it appeared so to conflict with results to which my studies had led me, that I determined to put his predictions to the test of trial.

The diagram on the blackboard (Fig. 1) is copied from

Fig. 1



his drawing given in "Science," Vol. IV, page 205, where is also found all that I shall quote from his address. The "Balance" consists, as you see, of four "Gyrostats," jointed at the four corners of the square, and enclosed in a metallic shell for the sake of hiding the interior. At the upper and lower corners are hooks, the upper one to support the apparatus, and the lower to support a weight. The index and scale are to enable the experimenter to measure any rise or fall of the weight.

The joints are as nearly frictionless as possible. The directions of the rotation of the fly-wheels are indicated by directional ellipses. Please to bear in mind that Sir William says, "The gyrostatic system might have been constituted of two gyrostatic members, but four are shown for the sake of symmetry." In other words, he declares that the effect would be exactly the same were either the right, or left hand pair omitted. This is fortunate, for in making such an instrument, the other two not only double the expense, but add very much to the difficulty of manipulation. I shall therefore content myself with one pair of wheels.

Before going further permit me to say a few words as to what a Gyrostat is, and how it differs from a Gyroscope. Sir William Thomson invented and named the former. Fig. 2, represents a section of it. (The original

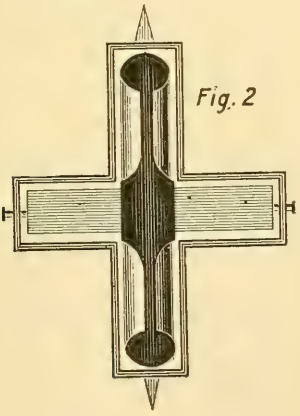


Fig. 2

drawing is shown in Nature, Vol. XV, page 297.) It is an ordinary gyroscopic wheel and axle enclosed in a sort of jacket or case, instead of being supported in a ring. The case answers all the purposes of the ring, but, besides, it permits a sort of circular knife edge to pass around the case in the plane of the wheel's equator. The

instrument stands on this knife-edge on a plate of glass or other hard and smooth plane.

It is evident that the sole characteristic difference between this and the gyroscope, is that in the former the point of support is under the centre of the wheel and in the equatorial plane, while in the latter, it is in the line of the axis produced. The so-called gyrostats in Sir W. Thomson's balance are supported at points in the axis, and therefore are gyroscopes. To make them in the form of gyrostats, *i. e.*, with jackets about them, would involve very considerable additional expense with no corresponding advantages. For this reason I have used in my apparatus the ordinary form, *i. e.*, a wheel supported in a ring.

As you see, the wheels in Sir W. Thomson's diagram (Fig. 1) all rotate in the same direction, *i. e.*, if a watch be placed at right angles to each axis, and at the end farthest from the upper hook, and with its back towards it, the rotation of each wheel is opposite to that of the hands.

This being so, Sir W. Thomson declares ;

1st. "That the system as a whole will have no moment of momentum ; in other words there will be no rotation around a vertical axis."

2d. "That in this condition of no rotation around a vertical axis, the index will, at first, oscillate up and down, until stopped by friction, and, but for the friction, it would, he says, vibrate forever."

3d. "If we check the vibration by hand, the weight will hang down at rest, the pin drawn out to a certain degree ; and the distance drawn out will be simply proportional to the weight hung on the lower hook, as in ordinary spring balances."

Now I propose to show you by actual trial that the system, as a whole, will have moment of momentum, and that there will be rotation around a vertical axis ;

"that the distance drawn out will *not* be simply proportional to the weight hung on the lower hook, as in ordinary spring balances," that, in fact, the weight will not hang down at all, but that the lower gyroscope will rise until its axis points vertically upward.

These are radically different results, and show that either nature, or Sir William Thomson, has a wrong conception of the action of this instrument.

My apparatus is on the table. It has as you see, only two gyroscopes, (Fig. 3) while his diagram has four, but you will recall his remark, "Only one set is needed, the other being merely for symmetry."

I had one with four wheels, but this one is much more easily manipulated.

At the top of the shaft (Fig. 3) is a saucer shaped de-

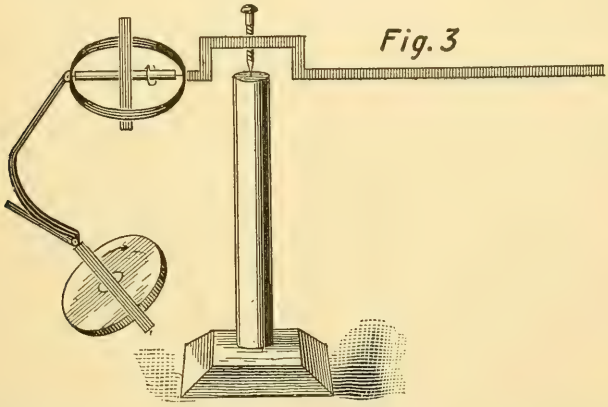


Fig. 3

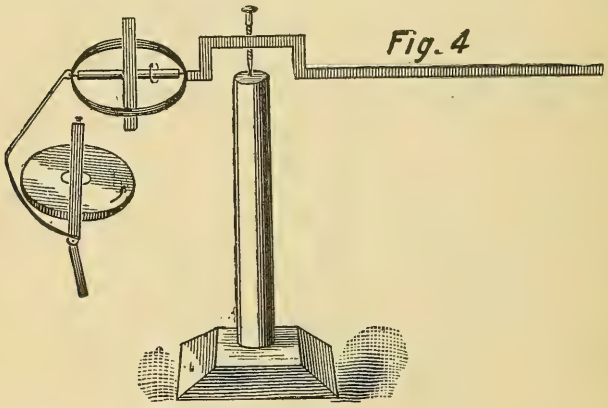
pression in which rests the conical point of an adjustable screw; both the point and the depression are of very hard and highly polished steel.

The two gyroscopes are connected by a stiff piece of sheet brass with almost frictionless hinges at each end. By this arrangement there is great freedom of movement in the vertical plane passing thro' both axles. On each gyroscope is a rod in the prolongation of its axis. These

give facility in handling the instruments, and, when desirable, movable weights can be attached to counterbalance, to any extent, the wheels. You have noticed, probably, that the rod on the lower gyroscope is bent out of line of the axle. This was done to permit the wheel to fall lower than it otherwise could.

I now wind a string on each axle in such a way that the direction of the resulting rotation is the same as in Sir William's diagram, and set both going. I will then place the instrument on the upright shaft, and leave it to itself. If his prediction is right there will be no rotation around the upright.

You see there is rotation. He says the lower end should fall, to a certain point, and there remain at rest. You see it rises till its axis points to the zenith. (Fig. 4.)



I will repeat the experiment. You see the results are the same.

I now wind the strings so that the wheels revolve in opposite directions, and place it in position as before.

The lower gyroscope instantly drops to its lowest possible place, I raise it, but it at once goes back.

At first it would seem as if there should be no moment of momentum. In fact while the lower gyroscope is

anywhere near the horizontal there is only the excess of influence of one of the gyroscopes over the other, but when it hangs down nearly vertical, it has no resisting power, and produces only the effect of so much weight hung on the other. I now set both in motion, the wheels rotating in opposite directions, the lower one drops down as you see, and the whole rotates somewhat rapidly around the shaft. I now stop the rotation of the lower wheel, the system revolves almost exactly as before.

It may possibly be thought that the statement in the Montreal address as to the direction of the wheels was a slip of the pen. But under it all and woven into it, is the belief that the peculiar sustaining power of a gyroscope is independent of the horizontal movement. It is impossible for one who thinks so, to have a knowledge of the "true inwardness" of gyrating bodies. For all sustaining power is absolutely gone the moment the horizontal movement ceases, and conversely the moment the nutation ceases, the precessional movement ends. By nutation I mean, of course, the falling or rising of the free end of the instrument, and by the precession, I mean the lateral movement.

As these statements are of great importance, I shall now prove their truth by experiment.

And, first, that the sustaining power ceases when the horizontal movement ceases, or, to use Sir William Thomson's words, when there is "no moment of momentum."

I set my instrument in motion. At one end is a pin projecting a short distance. I attach to a fixed standard a string which when horizontal is at the same height from the table as the gyroscope. As the instrument swings around, I catch the string on the pin. The instant it is drawn taut, the wheel drops. The so called gyroscopic power sustaining it, is gone.

In each of these, and in the previous experiments, the string was at right angles to the motion, and, conse-

quently, could have no effect in producing either of the stoppages.

To prove that when nutation ceases, precession ceases, I use the same apparatus and set the wheel in motion as before. Precession, as you see, begins at once. Now I pull upward by means of a cord on the free end of my gyroscope, just enough to stop the tilting downward. Instantly the precessional movement ceases. I slacken the string, it begins again. I pull upwards, and again it stops. Vary it as I may, when there is nutation, there is precession, and at no other time.

It is impossible to get the precession without the nutation, but it is easy to make the machine tilt up, or down, without the precession. When I pulled the string horizontally, there was no precession, but the wheel tilted down. Or if the wheel does not revolve on its own axis, gravity will readily cause it to tilt, but no lateral movement will occur. If the lever is overloaded so that that end is the heavier, the results are the same.

From these experiments we must conclude that nutation (or tilting) and precession are *not* mutually independent, and since precession is always a function of nutation and the rate of axial rotation, varying directly as the former and inversely as the latter, and since nutation is, at least sometimes, independent of both, we may reasonably regard nutation (or tilting) as the cause, and precession as the effect. This is true of the earth as well as of the gyroscope.

The following gentlemen were unanimously elected members of the Scientific Section, viz.: Silas Wodell, J. B. T. Tuthill, and Theodore Neumann.

Mr. Winne nominated for membership Mr. George B. Rogers.

APRIL 22, 1890—EIGHTY-THIRD REGULAR MEETING.

Edward Elsworth, chairman, presiding; a quorum present.

Mr. George B. Rogers was unanimously elected a member of the Scientific Section.

The Curator of the Museum, Dr. W. G. Stevenson, read the following report:

APRIL 22, 1890.

To the Scientific Section Vassar Brothers Institute:

The work of classifying and re-arranging the specimens in the Museum, begun a year ago, has been prosecuted during the past year with the most satisfactory results.

Some injured, imperfect, and redundant specimens have been removed, and all other specimens, except the mosses, fungi, coins, and commercial products, have been newly labeled and catalogued.

New white paper trays have been substituted for the old buff-colored trays heretofore used to hold the shells and fossils.

The Museum now presents an attractive appearance and the specimens are now available for purposes of study.

The work in the Museum during the past year has been mostly done by Mr. Gilbert Van Ingen—whose valued services are hereby gratefully acknowledged. Thanks are also given to Miss S. B. Arnold for valued services rendered in connection with the Museum work.

The additons to Museum during the year, are as follows by donation:

J. G. Vassar Estate,
Two *Canes.*

W. G. Stevenson,
One *Star fish,* Mt. Desert, Me.
Five *Strongylocentrotus dröbachiensis,* Mt. Desert, Me.

One *Frog*, Poughkeepsie, N. Y.
One *Obsidian*, Yellowstone Park.
One *Obsidian*, South Pacific.
One *Siliceous sinter*, Yellowstone Park.
Three *Native sulphur*, Yellowstone Park.
Four *Basaltic rocks*, Yellowstone Park.
Three *Siliceous sinter*, Yellowstone Park.
One *Smokey quartz and feldspar.*
One *Chalcedony geode*, Brazil.
One *Nut of Mangifera indica.*
Four *Fangs of rattlesnake.*
Three *sponges*, Nassau, N. P.

J. Wiggers,

One *Monstrous chick.*
One *White and brown rat.*

Miss Bockee,

One *Indian pestle.*

Ernest Pelton.

One *Chrysemys picta.*

Miss A. M. Goodsell, a collection of forty-two specimens, representing thirty-four species of marine shells from California.

One *Helix californiensis*, California.
One *Pupa*, South America.
One *Star*, Monterey, Cal.
One *Barnacle*, Monterey, Cal.
One *Strongylocentrotus dröbachiensis*, Marblehead, Mass.
One *Strongylocentrotus dröbachiensis*, Wood's Hole, Mass.
One *Silver carbonate*, Leadville, Col.

By Purchase :

One *Chrysemys picta.*
Two *Turtles.*
Five *Echinoderms.*
One *Chalybite*, Jefferson County, N. Y.
One *Zinc silicate*, Freedelsberg, Pa.
One *Quartz crystal*, Utah Terr.
One *Hexagonite*, St. Lawrence County, N. Y.
One *Lepidolite*, Massachusetts.
One *Actinolite*, Massachusetts.
One *Azurite*, Utah.

One *Brown tourmaline*, St. Lawrence County, N. Y.
One *Satin spar*, St. Lawrence County, N. Y.
One *Gold quartz*, California.
One *Hematite*, Jefferson County, N. Y.
One *Blue Calcite*, Jefferson County, N. Y.
One *Magnetite*, Port Henry, N. Y.
One *Bat.*
Two *Flying squirrels.*
Two *Quails.*
One *Woodcock.*
One *Red Squirrel.*
One *Red Fox.*
Two *Woodchucks.*
Three *Turtles.*

The following Indian implements and relics from John W. Emmet.

Three *Pestles.*
Two *Flint shell openers.*
Six *Axes.*
One *Totem.*
One *Chunkee-stone.*
Three *Weights.*
Three *Arrow points.*
One *Stone pipe.*
One *Bronze Pipe.*
One *Earthenware pot.*
One *Dog's head cut in stone.*
Three *Fossil shells* from Indian cave in Tennessee.
One *String wampum beads.*
One *String stone beads.*
Eighty-seven *Spear and arrow points*, all from Tennessee.

Total by donation	84
Total by purchase	149
Grand Total	233

A general summary of the specimens in the Museum classified, labelled and catalogued, may be stated as follows:

Fossils	420	Catalogue	Entries.
Minerals	346	"	"
Archæological	242	"	"
Mammals	31	"	"

Birds	285	Catalogue Entries.
Reptiles, Batrachians and Fish,	101	" "
Invertebrates	778	" "
Plants	1600	" "
Total	3803	" "

Of specimens displayed in cases, but not yet catalogued there are :

Woods	115
Fungi	53
Plants in alcohol	5
Total	173

In addition to the displayed, but uncatalogued specimens, there yet remain a large collection of mosses and fungi, polished woods, commercial products and coins to be arranged in the cases, and it is hoped that this work can be accomplished during the coming year.

Respectfully submitted,

W. G. STEVENSON,
Curator of the Museum.

The Librarian, Mr. Fred. S. Arnold, read the following report :

To the Scientific Section : The Librarian has the honor to announce that, during the preceding year, the library has been indebted to the following institutions for their publications :

Academy of Sciences of Philadelphia.
American Geographical Society.
American Institute of Mining Engineers.
American Museum of Natural History.
American Philosophical Society.
Arkansas Geological Survey.
Boston Society of Natural History.
California Academy of Sciences.
Cambridge Entomological Club.
Canadian Institute.

Chief of Engineers of United States Army.
Cincinnati Society of Natural History.
Colorado Scientific Society.
Cornell University Library.
Department of the Interior.
Franklin Institute.
Johns Hopkins University.
Kansas Academy of Sciences.
Meriden Scientific Association.
Minnesota Academy of Natural Sciences.
Museum of City of Milwaukee.
Museum of Wesleyan University.
New York Academy of Sciences.
New York Microscopical Society.
Nova Scotian Institute of Natural Science.
Oberhessichen Gesellschaft für Natur-und Heil-kunde.
Pennsylvania Geological Survey.
Royal Society of Canada.
Sociedad Científica Argentina.
Société Impériale de Moscow.
Society of Naturalists of Kiev.
School of Mines Quarterly.
United States Commission of Fish and Fisheries.
United States Department of Agriculture—Division of Entomology.
United States Geological Survey.
Washburn College Laboratory.
Wisconsin Academy of Science, Arts and Letters.

And that the library has also gratuitously received the "Open Court" of Chicago for six months, and is so receiving the "Mahabharata" as it is published.

The Institute has also purchased the "Manual of Conchology," by G. W. Tryon, continued by H. A. Pilsbury; and Scudder's "Butterflies of the Eastern United States and Canada."

The catalogueing of the library has been proceeding satisfactorily, but is not yet completed.

FREDERICK S. ARNOLD,
Librarian.

The Chairman of the Section presented the following report:

Members of the Scientific Section: It is a matter for congratulation that your chairman is able to report that much interest has been manifested in the meetings of the Section during the past year. They have been well attended, and the public interest has also been well maintained. Four new members have been elected during the year. The work of arranging and classifying the library, and making a comprehensive catalogue thereof has proceeded under the careful management of Miss S. B. Arnold. The museum under the skillful and intelligent supervision of the Curator, Dr. Stevenson, has made marked and important progress. With the assistance of Mr. Gilbert Van Ingen, the work of classifying and arranging the plants in their proper cases has been completed, and a catalogue made. The shells, including a large number of land shells have been identified, named and displayed in appropriate cases. The janitor reports a large attendance of visitors during the year, especially on Saturdays and holidays, which indicates that the people are appreciating our efforts. With renewed hopes for the future and a belief that with a little more effort we may make this Institute a living educational force in our city, I commend it and its interests again to the members who have so generously aided me in making the public meetings successful.

Respectfully submitted.

EDWARD ELSWORTH,
Chairman.

The following gentlemen were elected officers of the Section for 1890–1891:

Chairman, . . .	MR. EDWARD ELSWORTH.
Secretary, ...	MR. FRED. S. ARNOLD.
Curator, ...	W. G. STEVENSON, M. D.
Librarian, . . .	MR. CHARLES N. ARNOLD.

The Herbarium of Vassar Brothers Institute contains about 1,600 specimens, representing the flora of the United States, among which are the following specimens wholly collected in the County of Dutchess, in the vicinity of Poughkeepsie, N. Y.:

Name.	Locality.
Ranunculaceæ—	
Actæa alba, Bigel,	Poughkeepsie, N. Y.
Anemone Pennsylvania, L.,	" "
" Virginiana, L.,	" "
" "	" "
Clematis Virginiana, L.,	" "
" " "	" "
Ranunculus abortivus, L.,	" "
" var tricophyllus, G.,	" "
" " " "	" "
" bulbosus, L.,	" "
" fascicularis, Muhl,	" "
" "	" "
" multifidus, Pursh,	" "
" " "	" "
" recurvatus, Poir,	" "
" "	" "
" repens, L.,	" "
" "	" "
" sceleratus, L.,	" "
" "	" "
Thalictrum dioicum, L.,	" "
" polygamum, Muhl,	" "
Liriodendron tulipifera, L.,	" "
Anonaceæ—	
Menispermum canadense, L.,	Poughkeepsie, N. Y.
Berberidaceæ—	
Berberis aquifolium, Pursh,	Poughkeepsie, N. Y.
" vulgaris, L.,	" "
Caulophyllum thalactroides, Mx.	" "
Nymphæaceæ—	
Nymphæa adorata, L.,	Poughkeepsie, N. Y.
Papaveraceæ—	
Sanguinaria canadense, L.,	Poughkeepsie, N. Y.
Fumariaceæ—	
Dicentra cucullaria, D. C.,	Poughkeepsie, N. Y.

Cruciferæ—

Arabis candensis, L.,	Poughkeepsie, N. Y.	
" lævigata, D. C.,	"	"
" " "	"	"
" lyrata, L.,	"	"
Barbarea vulgaris,	"	"
Brassica rapa, L.,	"	"
Capsella bursa-pastoris (L.), Moench,	"	"
Cardamine hirsuta, L.,	"	"
" rhomboidea, D. C., C. Bulbosa, Schreb,	"	"
(Dentaria laciniata, Muhl), C. laciniata (Muhl), Wood,	"	"
Erysimum cheiranthoides, L.,	"	"
Lepidium campestre (L.), R. Br.,	"	"
" ruderale, L.,	"	"
" "	"	"
" virginicum, L.,	"	"
Nasturtium armoracia, Fries. (Armoracia rusticana, Rupp),	"	"
Sisymbrium officinale (L.), Scop,	"	"
Thalspi arvense, L.,	"	"

Violaceæ—

Viola blanda, Willd,	Poughkeepsie, N. Y.	
" " "	"	"
" var Muhlenbergii (Gray),	"	"
" palmata, L.,	"	"
" " "	"	"
" " var cucullata (Ait), Gray,	"	"
" pubescens, var eriocarpa, Nutt,	"	"
" " var Scarbriuscula, T. & G.	"	"
" rostrata, Muhl,	"	"
" " "	"	"
" sagittata, Ait,	"	"
" " "	"	"
" tenella, Muhl,	Mine Point, Poughkeepsie.	
(V. tricolor, L., var arvensis, D. C.)		

Polygalaceæ—

Polygala paucifolia, Willd,	Poughkeepsie, N. Y.	
" verticillata, L.,	"	"

Caryophyllaceæ—

Arenaria grœnlandica, Spreng,	Poughkeepsie, N. Y.	
" michauxi, Hook. (A. Stricta, Michx.)	"	"
Arenaria michauxi,	Camelot,	"

Cerastium arvense, L.,	Poughkeepsie, N. Y.
" viscosum, L.,	" "
Lychnis githago (L.), Lam.,	" "
Saponaria officinalis, L.,	" "
Silene, noctiflora, L.,	" "
Stellaria longifolia, Muhl.,	" "
" media, L., Smith,	" "
" pubera, Michx.,	" "

Illecebraceæ—

Scleranthus annuus, L.,	Poughkeepsie, N. Y.

Hypericaceæ—

Hypericum gentianoides, L. (H. Sarothra, Michx.),	Poughkeepsie, N. Y.
" gentianoides,	" "
" maculatum, Walt.,	" "
" mutilum. L.,	Camelot, "
" perforatum, L.,	Poughkeepsie, "

Malvaceæ—

Malva rotudifolia, L.,	Poughkeepsie, N. Y.

Linaceæ—

Linum usitatissimum, L.,	Poughkeepsie, N. Y.

Geraniaceæ—

Geranium maculatum, L.,	Poughkeepsie, N. Y.
Impatiens biflora, Walt. (I. fulva, Nutt.)	"
" oxalis, var stricta (L.), Sar.	"

Ilicineæ—

Ilex verticillata (L.), Gray,	Poughkeepsie, N. Y.
" " " "	" "

Celastraceæ—

Celastrus scandens, L.,	Poughkeepsie, N. Y.
" " "	" "
Euonymus atropurpureus, (Jacq)	"

Rhamnaceæ—

Ceanothus americanus, L.,	Poughkeepsie, N. Y.

Vitaceæ—

Vitis æstivalis, Michx.,	Poughkeepsie, N. Y.
" cordiflora, "	" "

Sapindaceæ—

Acer dasycarpum, Ehrh.,	Poughkeepsie, N. Y.
" Pennsylvanicum, L.,	" "
" " "	Amenia, "
" platanoides, L.,	Poughkeepsie, "

Acer pseudoplatanus, L.,	Poughkeepsie, N. Y.
" rubrum, L.,	" "
" spicatum, Lam.,	Wassaic, "
Staphylea trifolia, L.,	Poughkeepsie, "

Anacardiaceæ—

Rhus glabra, L.,	Poughkeepsie, N. Y.
" toxicodendron, L.,	" "
" "	Rochdale, "

Leguminosæ—

Amphicarpæa monoica, Nutt,	Poughkeepsie, N. Y.
Cassia Marilandica, L.,	" "
" nictitans, L.,	" "
" "	" "
Desmodium grandiflorum, D. C. (D. Acuminatum, D. C.)	" "
" lævigatum, D. C.,	" "
" nudiflorum, D. C.,	" "
" paniculatum, D. C.,	" "
" rotundifolium, D. C.,	" "
Lespedeza repens (L.), Bart.,	" "
" violacea, Pers.,	" "
Melilotus alba, Lam.,	" "
Robinia viscosa, Vent., (cultivated).	" "
Trifolium agrarium, L.,	" "
" pratense, L.,	" "
" procumbens, L.,	" "
Vicia tetrasperma, L.,	" "

Rosaceæ—

Agrimonia eupatoria, L.,	Poughkeepsie, N. Y.
Amelanchier Canadensis, L.,	" "
" " var oboralis, Mx.,	" "
" " " rotundifolia, T. & G.,	" "
Cratægus coccinea, L ,	New Hackensack, "
" " "	Poughkeepsie, "
" crusgalli, L ,	" "
" oxyacantha, L.,	" "
Cydonia japonica, L., (cultivated),	" "
" vulgaris, L.,	" "
Fragaria Virginiana, Ehr.,	" "
Geum album, Gmel.,	" "
" rivale, L.,	Amenia, "
" strictum, Ait,	Poughkeepsie, "

Geum Virginianum, L.,	Poughkeepsie, N. Y.	
" " "	"	"
Neillia opulifolia, L., (B. & H.),	"	"
Potentilla argentea, L.,	"	"
" arguta, Ph.,	"	"
" Canadensis, L.,	"	"
" " "	"	"
" tridentata, Soland,	Fishkill Mts.,	"
Prunus Pennsylvanica, L.,	Poughkeepsie,	"
" Virginiana, L.,	"	"
Pyrus Americana, D. C.,	"	"
Rosa blanda, Ait,	"	"
" Carolina, L.,	"	"
" lucida, Best (Ehr in part)	"	"
" rubiginosa, L.,	"	"
Rubus Canadensis, L.,	"	"
" occidentalis, L.,	"	"
" odoratus, L.,	"	"
" triflorus, Rich.,	"	"
" villosus, Ait,	"	"
Spiræa salicifolia, L.,	"	"
" sorbifolia, Schmidt,	"	"

Saxifragaceæ—

Chrysosplenium Americanum, L.,	Poughkeepsie, N. Y	
Mitella diphylla, L.,	"	"
Philadelphus grandiflorus, Willd,(cultivated),	"	"
Ribes aureum, Pursh, (cultivated),	"	"
" Cynosbati, L.,	"	"
" rubrum, L., (cultivated),	"	"
" uva-crispa, D.,	"	"
Saxifraga Virginiensis, Mx.,	"	"

Crassulaceæ—

Penthorum sedoides, L.,	Poughkeepsie, N. Y.

Lythraceæ—

Cuphea petiolata, (L.), Koehne. (Viscosissima, Jacq.)	Poughkeepsie, N. Y.	
" viscosissima,	"	"
Lythrum Salicaria, L.,	"	"
" " "	"	"
Nesæa verticillata, (L.), H. B. K.,	"	"

Onagraceæ—

Circæa Lutetiana, L.,	Poughkeepsie, N. Y.	
Epilobium coloratum, Muhl.,	"	"

Epilobium spicatum, (angustfolium), L.,	Fishkill Mts., N. Y.
" " "	Poughkeepsie, "
Ludwigia palustris, (L.), Ell.,	" "
Œnothera biennis, L.,	" "
" fruticosa, L.,	" "
" pumila, L.,	" "
" " "	" "

Cucurbitaceæ—

Sicyos angulatus, L.,	Poughkeepsie, N. Y.

Ficoideæ—

Mollugo verticillata, L.,	Poughkeepsie, N. Y.

Umbelliferæ—

Cryptotænia Canadensis, D. C.,	Poughkeepsie, N. Y.
Osmorhiza brevistylus, D. C.,	" "
" longistylus, D. C.,	" "
Pimpinella integerrima, B. & H.	" "
Sanicula Canadensis, L.,	" "
Thaspium aureum, L.,	" "
" " " (var trifoliatum),	" "

Araliaceæ—

Aralia nudicaulis, L.,	Poughkeepsie, N. Y.
" trifolia, D. & P.,	" "

Cornaceæ—

Cornus alternifolia, L. f.,	Poughkeepsie, N. Y.
" " "	" "
" Canadensis, L.,	Amenia, "
" circinata, L'Her.,	Poughkeepsie, "
" florida, L.,	" "

Caprifoliaceæ—

Lonicera ciliata, Muhl.,	Amenia, N. Y.
Symphoricarpus racemosus, Mx.,	Poughkeepsie, "
Viburnum acerifolium, L.,	" "
" dentatum, L.,	" "
" Lentago, L.,	" "
" prunifolium, L.,	" "
" pubescens, Pursh.,	" "
" " "	" "

Rubiaceæ—

Galium asprellum, Michx.,	Poughkeepsie, N. Y.
" circæzans, "	" "
" " "	" "
" lanceolatum, Torr.,	" "
" pilosum, Ait,	" "

Compositæ—

Achillea Millefolium, L.,	Poughkeepsie, N. Y.
Ambrosia Artemesiæfolia, L.,	" "
Anthemis cotula, L.,	" "
" arvensis, L.,	" "
Arctium lappa, L.,	" "
Artemisia biennis, Willd., (naturalized),	" "
Aster cordifolius, L.,	" "
" corymbosus, Ait.,	" "
" macrophyllus, L.,	" "
" Novæ-Angliæ, L.,	Livingston Station, "
" patens, Ait.,	Poughkeepsie, "
" Tradescanti, L.,	" "
Bidens bipinnata, L.,	" "
" " "	" "
" cernua, L.,	" "
" connata, Muhl.,	" "
" chrysanthemoides, Mx.,	" "
Chrysanthemum leucanthemum, L.,	" "
" "	" "
" " (var tubuliflorum),	" "
Coreopsis tinctoria, Nutt, (cultivated),	" "
Erechtites hieracifolia (L.), Raf.,	" "
Erigeron annuus (L.), Pers.,	" "
" bellidifolius, Muhl.,	" "
" Canadensis, L.,	" "
" Philadelphicus, L.,	Camelot, "
" ramosus, Walt. (E. Strigosus, Muhl.)	Poughkeepsie, "
Eupatorium ageratoides, L. f.,	" "
Gnaphalium decurrens, Ives,	" "
" polycephalum, Mx.,	" "
Helianthus decapetalus, L.,	" "
Hieracium paniculatum, L.,	" "
" venosum, L.,	" "
Krigia Virginica (L.), Willd.,	" "
Mikania scandens (L.), Willd.,	" "
Rudbeckia hirta, L.,	" "
" lacimata, L.,	Fishkill Mts., "
Senecio aureus, L.,	Poughkeepsie, "
" " var obovatus, Muhl,	" "
Solidago arguta, Ait.,	" "
" bicolor, L.,	" "
" " "	" "

Solidago cæsia, L., var axillatus, Gray,	Poughkeepsie, N. Y.
" lanceolata, L.,	" "
" nemoralis, Ait.,	" "
" squarrosa, Muhl.,	" "
" " "	" "
" ulmifolia, Muhl.,	" "
Sonchus arvensis, L.,	" "
" asper, Vill.,	" "
" oleraceus, L.,	" "
Taraxacum officinale, Web.,	" "
Tussilago farfara, L.,	Bangall, "
Vernonia Noveboracensis, L.,	Poughkeepsie, "
Xanthium strumarium, L.,	" "
Campanulaceæ—	
Campanula aparinoides, Ph.,	Poughkeepsie, N. Y.
" rotundifolia, L.,	" "
Specularia perfoliata, A. D. C.,	" "
Lobeliaceæ—	
Lobelia cardinalis, L.,	Poughkeepsie, N.Y.
" inflata, L.,	" "
" spicata, Lam.,	" "
" syphilitica, L., (white var),	" "
Vacciniaceæ—	
Gaylussacia resinosa (Ait.), T. & G.,	Poughkeepsie, N. Y.
Vaccinium Pennsylvanicum, Lam., var B. of N. Y. State Bot.,	" "
" Stamineum, L.,	" "
" vacillans, Soland,	" "
Ericaceæ—	
Andromeda polifolia, L.,	Amenia, N. Y.
Kalmia latifolia, L.,	Poughkeepsie, "
Rhododendron undiflorum, Torr.,	" "
" viscosum, Torr.,	" "
Primulaceæ—	
Lysimachia nummularia, L.,	Poughkeepsie, N. Y.
" quadrifolia, L.,	" "
" " "	" "
" terrestris, L.,	" "
" thyrsiflora, L.,	Amenia, "
Stieronema ciliatum, L.,	Poughkeepsie, "
Trientalis Americana (Pers.), Ph.,	Amenia, "
Oleaceæ—	
Jasminum fruticans, L., (cultivated),	Poughkeepsie, N, Y.
Ligustrum vulgare, L., (cultivated),	" "

Apocynaceæ—

Apocynum androseemifolium, L.,	Poughkeepsie, N. Y.
" " "	" "
" cannabinum, L.,	" "

Asclepiadaceæ—

Asclepias incarnata, L.,	Poughkeepsie, N. Y.
" exaltata, L. (phytolaccoides), Ph.,	Fishkill Mts., "
" purpurascens, L.,	Poughkeepsie, "
" quadrifolia, L.,	" "
" Syriaca, L., (cornuti, Decaisne),	" "
" verticillata, L.,	" "

Gentianaceæ—

Gentiana Andrewsii, Griseb,	Poughkeepsie, N. Y.
" " "	Stanford, "
" crinita, Frœl,	Poughkeepsie, "

Borraginaceæ—

Cynoglossum Morrisoni, L.,	Poughkeepsie, N. Y.
Echium vulgare, L.,	" "
Lithospermum arvense, L.,	" "
Myosotis palustris, Willd,	" "

Convolvulaceæ—

Cuscuta Gronovii, Willd.,	Poughkeepsie, N. Y.

Solanaceæ—

Datura Tatula, L.,	Poughkeepsie, N. Y.
Physalis viscosa, L.,	" "
Solanum Dulcamara, L.,	" "
" nigrum, L.,	" "

Scrophulariaceæ—

Chelone glabra, L.,	Poughkeepsie, N. Y.
Gerardia tenuifolia, Vahl.,	" "
Gratiola Virginiana, L.,	" "
Linaria vulgaris, Mill.,	" "
Melampyrum Americanum, Mx.,	Camelot, "
Mimulus alatus, L.,	Poughkeepsie, "
Pedicularis Canadensis, L.,	" "
Pentstemon pubescens, Soland.,	" "
Scrophularia nodosa, L., (var Marilandica, Gray),	" "
Verbascum Thapsus, L.,	" "
Veronica Americana, Schw.,	" "
" arvensis, L.,	" "
" officinalis, L.,	" "
" peregrina, L.,	" "
" scutellata, L.,	" "

Veronica serpyllifolia, L., ... Poughkeepsie, N. Y.
" Virginica, L., . . Cedar Ridge, " "

Orobanchaceæ—
Amphyllon uniflorum, T. & G., . . . Poughkeepsie, N. Y.

Acanthaceæ—
Dianthera Americana, L., ... Poughkeepsie, N. Y.

Verbenaceæ—
Phryma Leptostachya, L., ... Poughkeepsie, N. Y.
Verbena urticifolia, L., ... " "

Labiatæ—
Brunella vulgaris, L., ... Poughkeepsie, N. Y.
Calaminthra Clinopodium, Benth., . . " "
Galeopsis Tetrahit, L., ... " "
Hedeoma pulegioides, L., ... " "
Isanthus cœruleus, Mx., ... " "
Leonorus cardiaca, L., ... " "
Lycopus Europœus, L., ... " "
" Virginicus, L., ... " "
Nepeta cataria, L., ... " "
" hederacea, L., ... " "
Scutellaria galericulata, L., ... " "
" nervosa, Ph., ... " "
Stachys aspera, Mx., ... " "
Trichostema dichotomum, L., . . . " "

Plantaginaceæ—
Plantago lanceolata, L., ... Poughkeepsie, N. Y.
" rugelli, Dec., ... " "

Amarantaceæ—
Amarantus retroflexus, L., ... Poughkeepsie, N. Y.

Chenopodiaceæ—
Chenopodium album, L., ... Poughkeepsie, N. Y.
" " (L.,) var variide, G., . " "
" bonus-henricus, L., . . " "
" Botrys, L,, ... " "
" hybridum, L., . . . " "
" urbicum, L., ... " "

Phytolaccaceæ—
Phytolacca decandra, L., ... Poughkeepsie, N. Y.

Polygonaceæ—
Fagopyrum esculentum, Moench, . . Poughkeepsie, N. Y.
Polygonum acre, H. B. K., ... " "
" amphibium, L., ... Amenia, "

Polygonum arifolium, L.,	Poughkeepsie, N. Y.
" aviculare, L.,	" "
" convolvulus, L.,	" "
" Hydropiper, L.,	" "
" Pennsylvanicum, L.,	" "
" Persicaria, L.,	" "
" sagittatum, L.,	" "
" tenue, Mx.,	" "
Rumex crispus, L.,	" "
" obtusifolius, L.,	" "
Aristolochiaceæ—	
Asarum Canadense, L.,	Poughkeepsie, N. Y.
Lauraceæ—	
Sassafras officinale, Nees,	Poughkeepsie, N. Y.
Thymelaceæ—	
Dirca palustris, L.,	Poughkeepsie, N. Y.
" " "	Amenia, "
Euphorbiaceæ—	
Acalypha Virginica, L.,	Poughkeepsie, N. Y.
Euphorbia hypericifolia, L.,	" "
" maculata, L.,	" "
Urticaceæ—	
Bœhmeria cylindrica, L.,	Poughkeepsie, N. Y.
Cannabis sativa, L.,	" "
Pilea pumila, L.,	" "
Ulmus Americana, L.,	" "
" fulva, Mx.,	" "
Juglandaceæ—	
Carya alba, Nutt.,	Poughkeepsie, N. Y.
Juglans cinerea,	" "
Cupuliferæ—	
Alnus serrulata, Willd,	Poughkeepsie, N. Y.
Betula alba, L., var populifolia, Marsh,	" "
Carpinus Americana, Mx. Caroliniana, Walt.,	" "
Quercus alba, L.,	" "
" bicolor, Willd,	" "
" ilicifolia, Wang.,	" "
" Prinus, L.,	" "
" rubra, L.,	" "
Salicaceæ—	
Salix alba, L., var vitellina (L.), Koch.,	Poughkeepsie, N. Y.
" cordata, Muhl.,	" "
" humilis, Muhl.,	" "

Salix nigra, Marsh,	Poughkeepsie, N. Y.
" purpurea, L.,	" "
" rostrata, Rich.,	" "
" sericea, Marsh,	" "
Hydrocharidaceæ—	
Anacharis Canadensis, Plan.,	Poughkeepsie, N. Y.
Vallisneria spiralis, L.,	" "
Orchidaceæ—	
Cypripedium pubescens, Willd,	Poughkeepsie, N. Y.
Goodyera pubescens, R. Br.,	" "
Habenaria flava, L.,	" "
" Hookeriana, Torr.,	Amenia, "
" lacera, Mx.,	Stanford, "
Orchis spectabilis, L.,	Poughkeepsie, "
" " "	Amenia, "
Spiranthes cernua, Rich.,	Poughkeepsie, "
" gracilis, Bigelow,	" "
Iridaceæ—	
Iris ochroleuca, L., (spontaneous),	Poughkeepsie, N. Y.
" versicolor, L.,	" "
Sisyrinchium anceps, Car.,	" "
Amaryllidaceæ—	
Hypoxys erecta, L.,	Poughkeepsie, N. Y.
Dioscoreaceæ—	
Dioscorea villosa, L.,	Poughkeepsie, N. Y.
Liliaceæ—	
Chamælirium luteum, L.,	Amenia, N. Y.
Erythronium Americanum, Ker.,	Poughkeepsie, "
Lilium Canadense, L.,	" "
Majanthemum Canadensis, Desf.,	" "
Medeola Virginica, L.,	" "
Oakesia sessilifolia, (L.), Watson,	" "
Polygonatum biflorum, Walt.,	" "
Smilacina racemosa, L.,	" "
Smilax herbacea, L.,	" "
" rotundifolia, L.,	" "
Trillium erectum, L.,	Amenia, "
Uvularia perfoliata, L.,	Poughkeepsie, "
Veratrum viride, Ait.,	" "
Pontederiaceæ—	
Pontederia cordata, L.,	Poughkeepsie, N. Y.
Juncaceæ—	
Luzula campestris, L.,	Poughkeepsie, N. Y.

Aroidea—

Acorus calamus, L., ... Poughkeepsie, N. Y.
Arisæma triphyllum, L., ... " "
Orontium aquaticum, L., ... " "

Lemnaceæ—

Spirodela polyrrhiza, Schleid., . . . Poughkeepsie, N. Y.

Alismaceæ—

Sagittaria graminea, Mx., . . . Wappingers Creek, N. Y.
" variabilis, Eng., ... Poughkeepsie, "

Naiadaceæ—

Potamogeton natans, L., ... Poughkeepsie, N. Y.
" pauciflorus, Pl., . . . " "

Cyperaceæ—

Eriophorum polystachyon, L., ... Amenia, N. Y.

Gramineæ—

Panicum latifolium, L., ... Amenia, N. Y.

Coniferæ—

Juniperus communis, L., ... Poughkeepsie, N. Y.
" Virginianus, L., ... " "
Pinus rigidus, Miller, ... " "
" strobus, L., ... " "

Filices—

Aspidium acrostichroides, Swz., . . . Poughkeepsie, N. Y.
" " var incisum, Gray, " "
" bootii, Tuckm., ... " "
" cristatum, Swz., ... " "
" " var Clintonianum, Eaton, " "
" marginale, Swz., ... " "
" novaboracense, Swz., . . . " "
" spinulosum, Swz., . . . " "
" " var intermedium, D. C. C., . . " "
" thelypteris, Swz., . . . " "
Asplenium ebeneum, Ait., ... " "
" filix-fœmina, Bernh., . . . " "
" ruta-muraria, L., . . . " "
" thelypteroides, Mx., . . . " "
" trichomanes, L., ... " "
Camptosorus rhizophyllus, Link., . . " "
" " " . . . Bangall, "
Cheilanthes vestita, Swz., ... Poughkeepsie, "
Cystopteris bulbifera, Bernh., . . . " "
" fragilis, Bernh., ... " "

Dicksonia pilosiuscula, Willd.,	Poughkeepsie, N. Y.
Onoclea sensibilis, L.,	" "
" struthiopteris, Hoffm.,	" "
Osmunda cinnamomea, L.,	" "
" Claytoniana, L.,	" "
" regalis, L.,	" "
Pellæa atropurpurea, Link.,	" "
Phlegopteris hexagonoptera, Fee.,	" "
Polypodium vulgare, L.,	" "
Pteris aquilina, L.,	" "
Schizæa pusilla, Ph.,	Manchester, "
Woodsia ilvensis, R. Bs.,	Poughkeepsie, "
" obtusa, Torr.,	" "

Ophioglossaceæ—

Botrychium lanceolatum, Aug.,	Pine Plains, N. Y.
" matricariæfolium, A. Br.,	" " "
" ternatum, Swz.,	Poughkeepsie, "
" " (var intermedium),	" "
" " (var obliquum, Milde.),	" "
" " " "	Stanford, "
Ophioglossum vulgatum, L.,	Pine Plains, "

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